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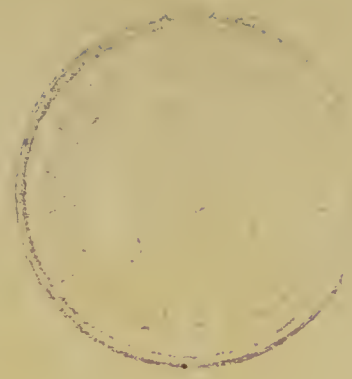
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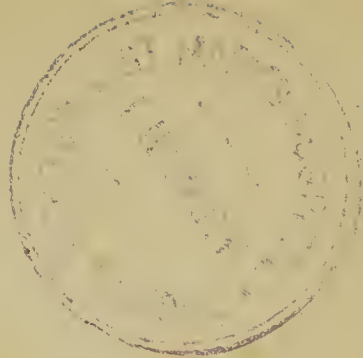
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SCIENCE PROGRESS

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Science Progress

A QUARTERLY REVIEW

OF

CURRENT SCIENTIFIC INVESTIGATION

CONDUCTED BY

SIR HENRY BURDETT, K.C.B.

EDITED BY

J. BRETLAND FARMER, M.A.

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No. 6.

WHY WE MEASURE PEOPLE.

SOME Anthropologists go about the country with little cards in their hands on which they jot a memorandum of the colour of the hair and eyes of the people they come across. Others, with simple appliances, measure the stature and the span, the length and breadth of the heads, of the faces and of the noses of those who will submit to it. Many are the devices adopted and varied are the arguments employed to induce the country folk to allow themselves to be measured. Generally it is best to commence with the stature, as if it were wanted merely to find out who were the tallest men; most people take a pride in having large heads, and so vanity paves the way for the cephalic measurements. The subject then becomes interested and amused and the onlookers indulge in mild chaff, so that by one means or another not only can the above-mentioned measurements be taken, but others may be added. When the eye and hair colours and other physical features are noted, a very fair documental description of the individual has been secured. The subject is generally ready enough to be photographed, both full-face and side view, and a promise of a copy of his photograph will usually induce a recalcitrant person to submit to the entire operation.

The question not unnaturally presents itself to onlookers—Why are these observations and measurements made?

Now there are several reasons why people are measured. In most anthropometric laboratories the object is mainly to test the growth and physical fitness of people, so that they may see how they stand in relation to other people, and to

discover certain physical imperfections. M. Alphonse Bertillon, some twenty years ago, devised an excellent series of measurements for the identification of criminals. It will be obvious that a precise and rapid method of identification not only expedites justice and saves expense, but at the same time it is a safeguard to the prisoner, as it prevents his being punished for the crimes of others. It is really beautiful to see the celerity and certitude of the Bertillon system, and it is satisfactory to think that it is now, though tardily, being adopted in the British Islands. This system of identification is one of the practical applications of anthropology to ordinary life, and its utility is beyond question.

Another reason for measuring people is to endeavour to trace their racial characters and affinities. This is why peripatetic observations are made, and why an influential committee has been appointed by the British Association for the Advancement of Science to conduct an Ethnographical Survey of the United Kingdom.

The work done by this committee will be found in the Reports of the Association, but as yet no systematic survey of the British Islands has been attempted. The Ethnographical Survey of Ireland has been undertaken by a Dublin committee, which is supported by the Royal Irish Academy, and four comprehensive reports¹ have been published by that body. These reports are drawn up on the following lines:—

I. *Physiography of the district investigated.* II. *Anthropography.*—1. Methods; 2. Physical characters with lists of measurements; 3. Vital Statistics (general and economic), (A) Population, (B) Acreage and Rental, (C) Language and Education, (D) Health; 4. Psychology; 5. Folk names. III. *Sociology.*—1. Occupations; 2. Family Life and Cus-

¹ "The Ethnography of the Aran Islands, County Galway," by Prof. A. C. Haddon and Dr. C. R. Browne, *Proc. Roy. Irish Acad.* (3rd ser.), ii., 1893, pp. 768-830, pls. xxii-xxiv; "The Ethnography of Inishbofin and Inishshark, County Galway," by Dr. C. R. Browne, *l. c.*, iii., 1894, pp. 317-370, pls. viii., ix.; "The Ethnography of the Mullet, Inishkea Islands and Portacloy, County Mayo," by Dr. C. R. Browne, *l. c.*, iii., 1895, pp. 587-649, pls. xv.-xvii.; "The Ethnography of Ballycroy, County Mayo," by Dr. C. R. Browne, *l. c.*, iv., 1897, pp. 74-111, pls. iii., iv.

toms ; 3. Food ; 4. Clothing ; 5. Dwellings ; 6. Transport. IV. *Folk-Lore*.—1. Customs and Beliefs ; 2. Legends and Traditions. V. *Archæology*.—1. Survivals ; 2. Antiquities. VI. *History*. VII. *Ethnology*. VIII. *Bibliography*.

It will be evident that this is a somewhat ambitious programme, and although in many instances the information given on a particular subject is meagre, owing to the very limited time available for work in the field, it was considered best to keep to the general scheme in order to emphasise the fact that in all investigations of this kind the widest possible outlook must be kept.

The late Rev. Dr. Walter Gregor has accomplished some very good work for the committee, especially in the folk-lore and physical characters of the natives of Wigtownshire and Kircudbrightshire. This south-west corner of Scotland is ethnologically important, as it was the stronghold of the Picts, a people about whom very contrary opinions exist. A preliminary "Report on Folk-lore in Galloway, Scotland," by the Rev. Walter Gregor, LL.D., was published in the *Report of the British Association* for 1896 (p. 612), and an abstract of the measurements made on the Galloway folk will be printed in the *Report of the Association* for 1897. A little work has been done on the physical features of the inhabitants of some of the villages near Cambridge ; the main results will also be published in the 1897 Report. Other investigations of a similar nature are now being made under the auspices of the committee.

It should never be forgotten that these researches owe their inspiration to the laborious investigations of the revered Dr. John Beddoe on the hair and eye-colour of the inhabitants of the British Islands. His results were published in 1885 in his *Races of Britain*, a book which is a mine of information. Dr. Beddoe also made numerous observations of a similar kind on the continent.

In 1883 the Anthropometric Committee of the British Association published their final report. The committee was appointed for the purpose of collecting observations on the systematic examination of the height, weight, span, chest girth, breathing capacity, strength, colour-blindness,

eyesight, colour of eyes and hair of the inhabitants of the British Isles. Several of these may be of interest, but they are of no special ethnological importance. This report was drawn up by Mr. C. Roberts and Sir Rawson W. Rawson. And it is illustrated by a few maps showing the distribution of height, weight and certain combinations of the colours of eyes and hair.

In order to gain definite information respecting the racial affinities of a people it is necessary to have other measurements than the above-mentioned. A happy example



FIG. I. SKETCH MAP OF THE DISTRICT.

of the value of employing different sets of observations is afforded by the brilliant work done Dr. R. Collignon in his studies on the anthropology of France. His paper on the five Departments of Dordogne, Charente, Corrèze, Creuse and Haute-Vienne (*Mém. Soc. d'Anth.*, Paris, i. (3^e sér.), 3^e fascic., 1894) constitutes so instructive an example of the modern methods of Anthropological investigation and of the lessons to be learnt from them that an abstract of it will furnish the best answer to the question: Why are these observations and measurements made?

The region under consideration consists partly of the calcareous beds, and partly of primitive rocks of the Central Plateau of France; the limiting line between them is shown on map No. 1; to the east it passes into the mountainous mass of Auvergne. The five Departments which constitute this region are traversed from east to west by the gradually decreasing elevations of the Limousin Mountains, which serve as barriers between the three basins of the Dordogne—or rather of its right affluents the Dronne Isle, Vézère and Corrèze; of the Charente and of the left affluents of the Loire, the Vienne, Gartempe, Creuse and Cher. A line running roughly north and south, starting at the junction of Charente and Haute-Vienne and passing not far to the east of Périgueux, would separate the fertile district to the west from the poor lands to the east. At certain points in the latter, as in the Limousin, the valleys are rich, but the uplands are infertile, and produce only chestnuts and scanty cereals.

The physical features of the population studied by Dr. Collignon are mainly those of the conscripts for the xii^e Corps d'armée who are recruited from these five Departments.

The characters are given in the order of importance that Dr. Collignon allocates to each.

CEPHALIC INDEX.

This index is the ratio of the breadth of the head to its greatest length, the latter being taken as 100. In dealing with skulls, anthropologists usually arrange the indices in three groups: (1) Dolichocephals, with an index of less than 75; (2) Mesaticephals, with an index between 75 and 80; and (3) Brachycephals, having an index of over 80. It is the practice of some anthropologists to deduct two units from the corresponding index of the living head so as to reduce the cephalic to the cranial index.

There is a tendency at present not to lay too much stress upon these purely empirical divisions and some

would raise the upper limit of dolichocephaly two or three units.

The following table gives the distribution of the cephalic indices in the five Departments, in the case of Dordogne a further analysis is made which proves that the southern part of that Department is much more brachycephalic than the northern. The mean index of this Department, if alone considered, gives extremely little information.

CEPHALIC INDEX—PROPORTION PER CENT.

	Charente.	Haute-Vienne.	Creuse.	Corrèze.	Dor-dogne.	North Dor-dogne.	South Dor-dogne.
67-69	·17				·22	·30	
70-74	5·60	3·78	·58		5·95	8·24	
75-79	41·95	37·81	19·24	6·90	38·30	48·53	11·54
80-84	41·82	42·86	62·38	43·27	39·46	37·63	44·22
85-89	10·85	14·71	16·92	41·73	13·83	5	39·92
90-94	·50	·84	·88	7·93	2·13	·30	6·93
95-97				·17	·11		·39
Mean index	80·43	80·93	82·16	84·93	80·70		

It is evident that this table indicates considerable differences in the ethnic constitution of each Department. Taking the extremes, we have on the one hand, North Dordogne with its 8·5 per cent. of indices below 75, or Charente with 5·7 and only ·5 of ultrabrachycephals and Corrèze on the other which has no dolichocephal below 75, but has 8·1 per cent of indices over 90.

Taken as they stand, the great majority of these indices fall into the brachycephalic division, while very few are dolichocephalic.

The mean index of the French population being 83·57, Dr. Collignon, in order to simplify matters, describes as brachycephals those indices above 83. The cantons which come under this grouping form a compact mass to the south and south-east as is seen in the accompanying map. To the north there are two islands in which the index does not exceed 83·8.

Inversely, and as a matter of convenience, he regards as dolichocephalic all the regions in which the index is less than 80. Two large groups of dolichocephalic cantons are isolated by this means; the more important covers two-thirds of the department of Dordogne (the valleys of the Isle and of the Dronne) and about one half of Charente, mainly to the south and south-east. The other has Limoges for a centre and the seven cantons that surround it.

In the narrow band of country between these two groups the index is 81.



*FIG. 2. THE DISTRIBUTION OF THE CEPHALIC INDEX.

Areas with an index of less than 80, shaded; those between 80 and 83, left blank; those over 83, cross-hatched.

This clearly defined distribution is of the greatest importance, for alone it provides a key to the local ethnography.

Another point not less worthy of attention is the clear manner in which these two head types are separated:

1. between the two Departments of Dordogne and Corrèze;
2. between the two portions of Dordogne which are separated by the rivers Vézère and Dordogne.

As a matter of fact the boundary between the two

Departments of Dordogne and Corrèze was formerly precisely that between Périgord and Limousin, and in earlier times between the Petrocorii and Lemovices. To the right of this entirely conventional frontier the indices run from 85·4 to 87·3, while to the left they vary from 78·7 to 81·4, but there is nothing in history to explain this discrepancy. The explanation appears to be that well before the Conquest the two peoples differed in race, the one being what Cæsar called Celts, the other probably belonging to the people whom he named Aquitainians.

The southern portion of Dordogne is also brachycephalic and Celtic, and so Dr. Collignon is inclined to think that it did not form part of the territory of the Petrocorii, but that it should be divided among the Nitiobriges and Cadurci, whose equally brachycephalic descendants still people Lot-et-Garonne and Lot. Another line of evidence supports this conclusion. It is known that the primitive episcopal dioceses corresponded to the territories of the ancient Roman *civitates*, since a bishop was established in each city by the emperors. Whilst the northern, eastern and western frontiers of the diocese of Périgux correspond very closely with that of the modern Department, the region south of the Vézère belongs to the Bishop of Cahors, which tends to show that the natives of the south of Dordogne are the descendants not of the Petrocorii, but of the Cadurci.

The differences between the two parts of Limousin, of which the one forms part of Corrèze and the other the south of Haute-Vienne, can be explained in an analogous manner. The former is brown and brachycephalic, while the latter is fair and dolichocephalic.

One may well believe that the Lemovices, those of the neighbourhood of Limoges, were no more Petrocorii than Celtæ, but a fair people of Belgic or Germanic origin, established in Celtica, who had over-lorded the ancient brachycephalic people who there preceded them.

Inversely, Briva-Curetia, another old Gaulish town of Limousin, was the centre of gravitation of the first inhabitants, if not their capital.

In Charente there is only one canton in which the mean index rises over 83. In this canton of Chabanais is the small village of Chassenom on the left bank of the Vienne. It is interesting to see the old Celtic race here, preserved with a relative purity, still grouped around the ruins of its oppidum (Cassinodunum), where, compared with the rest of the Department, it appears as an island surrounded by the combined flood of brown and fair dolichocephals.

COLOUR OF THE HAIR AND EYES.

A statistical inquiry concerning the distribution of the colours of the eyes and hair leads to the following results. The browns predominate markedly over the blonds. But for a group of cantons in Creuse all the district should be ranged under the brown or moderately brown categories.

In the following table the numbers are in relation to 100, the differences between 100 and the fairs and the darks represent the eyes and hair of intermediate tint :—

	Eyes.		Hair.			Half sum of eyes and hair.		Excess of dark over light.
	Blue.	Dark.	Fair.	Dark and black.	Black only.	Light.	Dark.	
Haute-Vienne	36·7	24·6	21·8	49·6	5·25	29·2	37·1	7·9
Creuse -	34·7	23·3	21·9	53·9	6·12	28·3	38·6	10·3
Charente -	33·8	23·6	17·2	57·6	5·80	25·5	40·6	15·1
Corrèze -	29·5	23·3	15·4	58·4	3·80	22·3	40·9	18·6
Dordogne -	34·2	23·6	15	66·3	12·05	24·6	45·0	20·4

On comparing this table with the map it will be seen that although Dordogne has an absolute greater number of blonds than Corrèze it is relatively darker, owing to the fact that the darks are greatly in excess in certain cantons ; in other words, Dordogne is more patchy and Corrèze more uniform in the distribution of their hair and eye colours. It is evident in using the word blond this term is employed in only a relative sense. It is with this reserve and for the sake of convenience that the term blond will be employed. In the most blond group, that in the neighbourhood of

Aubusson in Creuse, the blonds amount to only 33·6 per cent.—that is to say, one-third.

In order to gain a clear conception of the distribution of the hair and eye colours, it will be simpler to assume the whole region as originally inhabited by a brown population and then to follow the probable route of the blonds.

The most important spot where the blond type is best preserved is the east of the Department of Creuse, especially the plateau of Gentioux and the upper basin of the river Cher and of its left affluents.



FIG. 3. THE DISTRIBUTION OF COMBINED HAIR AND EYE COLOUR.

Excess of Browns from 0 to 10, shaded ; 10 to 30, blank ; over 30, cross-hatched.

The second relatively blond region has Limoges for its centre. In certain spots the type is preserved with a remarkable purity, particularly among the women. Dr. Collignon was very much struck with the resemblance of these to the women of Contentin in Normandy. It appears that the blonds radiate from Limoges in four directions: 1, towards the north in the direction of the old Roman road of Argentomagus and Avaricum (Argentan and Bourges), later the route to Paris, that is to say, along the road which united this town with the great blond centres

of the North of France ; 2, towards the east where it joins with blonds of the Cher region ; 3, to the west in the direction of Angoulême ; and 4, southwards towards Périgueux.

The third route of blond immigration would be the route from Paris to Bordeaux through Angoulême.

Limoges formed a centre, and towards the four points of the compass lay four very ancient and important towns, Avaricum (Bourges), Gergovia (Clermont), Vesuna (Périgueux), and Ecolisma (Angoulême). The latter town was the only one of the four that was not united to Limoges either by a Roman road of the first order, or later by a postal route ; and we find in the region between these towns the blonds are deficient. The importance of the communications between Limoges and Bordeaux through Périgueux is affirmed by the long line of blonds which occur along that route. To take a biological simile, Limoges represents a ganglion protruding its nerve fibres in all directions towards other similar ganglia.

The distribution of black hair is worthy of note. In Dordogne it is marked in *la Double*, in the valleys of the rivers Dordogne, Isle and Dronne. Secondary centres extend towards the north of Charente and of Creuse. There is thus a current inverse to that of the blonds. The great pressure of blonds came from the north-east and from the north ; it traversed the district obliquely in a north-east to south-west line. Inversely the black-haired race appears to be massed in the south-west, and to be distributed, with a gradually decreasing importance, towards the north-east and north.

STATURE.

The measurements of the stature are not so instructive from a racial point of view as might have been expected.

All the tall statures are massed at the circumference of the four Departments of which the statistics are available, with the exception of an important centre about Limoges. In the map the distribution of the heights over 1640 mm. (5 ft. 4½ in.) is shown by the vertical lines. In mapping

the distribution of the statures under 1610 (5 ft. 3½ in.) it is seen that besides several scattered areas towards the south of the district under discussion there is a large central area, which, following the example of Broca who found a similar area of a dwarfed population in Basse Bretagne, Dr. Collignon calls "the Limousin black spot" (*"la tache noire limousin"*).

In other cantons less than 10 per cent. of the statures have under 1600 (5 ft. 3 in.), those in the black spot have:



FIG. 4. THE DISTRIBUTION OF STATURE. (The Department of Creuse is omitted.)
Stature less than 161 cm. (5 ft. 3½ ins.), cross-hatched; between 161 and 164 cm., blank; from 164 (5 ft. 4½ ins.) to 166 cm. (5 ft. 5½ ins.), shaded.

The line AB separates the granites and crystalline rocks on the east from the calcareous beds on the west.

without exception over 30 per cent.; eight cantons have more than 40 per cent., one has 54·7 per cent. while that of Saint Mathieu has 67·6 per cent. less than 1600, four below 1540, and 8·8 per cent. below 1500 (4 ft. 11 in.)! True dwarfs, that is those with a stature below 1500 mm., are exceptional everywhere.

These figures are not due to an accidental and temporary selection as the following figures of Bondin prove, which extended over a period of thirty years (1831-1860). These

tables show that Dordogne, Corrèze and Haute-Vienne are among the four Departments in the whole of France which have the greatest number of exemptions from conscription owing to deficiency of stature. The neighbouring Department of Puy-de-Dôme occupies the eighty-fourth rank with 128 exemptions.

	Exemptions per 1000.	Rank in France.		Heights of 1732 per 1000.	Rank in France.
Creuse - -	89	63	Creuse - -	44	74
Charente -	114	82	Corrèze - -	43	77
Dordogne -	124	83	Charente -	41	79
Corrèze - -	168	85	Dordogne -	39	80
Haute-Vienne	175	86	Haute-Vienne	31.6	86

Inversely the high statures are also at a minimum. Haute-Vienne having the least proportion of tall people and the greatest population of short people of any Department of France.

Bondin and Broca considered that this remarkable shortness was purely a question of race, the normal smallness of the brachycephals. This very simple explanation will no longer suffice, in the presence of the dolichocephaly proved for Dordogne, Charente and Haute-Vienne. If we compare the maps of the distribution of the cephalic index with those of colour and stature, and mentally superimpose them, we find that there is absolutely not a shadow of a relation between them. The "black spot" extends alike over the brachycephals of Corrèze, the brown dolichocephals of Dordogne, and the fair dolichocephals of Haute-Vienne. There is then no relation between this demonstrated phenomenon and race.

Some anthropologists seek a cause in the geological character of the soil; but here as in Brittany and Cotentin it explains nothing. It is true that the line of separation between the granites and crystalline rocks on the east and the calcareous beds on the west runs pretty closely along the southern border of the black spot; but we also find the greatest number of high statures on the granites and the

low statures flourish equally well on the Liassic and Cretaceous calcareous beds of Sarladais and Riberaois.

The only plausible explanation is the social condition, and in this case it is summed up in the expressive French term *la misère*. The steep slopes and barren soil only produce poor cereals, rye, barley and buckwheat. The natives live on these and on milk and chestnuts. Communication is difficult; no great tillage as in the fertile valleys of the Vienne and Gartempe, none of the larger industries that enrich a people. "In the cantons of Vigeois, Uzerche and Treignac in Corrèze" writes M. Vacher, "the population is settled in confined valleys, in deep gorges receiving little light and air, with an impermeable subsoil and marshy ground." In a poor country the most elementary hygiene is unknown, the death rate is raised and organic defects are more frequent than elsewhere. One of the more direct corollaries of misery is ignorance. In many other parts of France, as in the Hautes-Alpes and Sologne, poverty is allied with ignorance and results in the degeneration of the race.

THE NASAL INDEX.

The nasal index is the ratio of the breadth of the wings of the nose to its length, the latter being measured from the root of the nose to where the septum passes into the upper lip. The narrow noses (leptorhines) are those with an index below 70; the mesorhines range from 70 to 85; while the broad noses (platyrrhines) are those above 85.

The mean nasal index is 68·8, but the individual range is enormous, 49·9 to 96·4, that is more than 46 units. As a whole the mesorhine indices, *i.e.*, those over 70 are massed in the centre of the five Departments.

This distribution follows in the main that of the stature. But why? Simply in accordance with a law previously thus formulated by Collignon: "In a given race, leptorhiny is in direct relation to stature; the higher this is raised the longer the nose, the lower the height the more the nose tends to mesorhiny".¹

¹ "Etude anthropométrique élémentaire des principales races de France." *Bull. Soc. d'Anthrop.* Paris, 1883, p. 508.

A careful consideration of the data tends to show that, independent of stature, the brachycephals possess a mean nasal index of about 69, that is to say very near mesorhiny, which is in agreement with previous investigations. The dolichocephalic races are more leptorhine.

One result of this inquiry is that the value of the nasal index has received a serious blow. Certainly this character is very important for the discrimination of the great trunks of mankind, as has been abundantly proved in anthropological investigations in India, but so far as the European peoples are concerned, it is incontestable that the nasal index has only a subsidiary and relative value.

HEIGHT INDICES OF THE CRANIUM.

The importance of the vertical height of the cranium as a racial character has been emphasised by Virchow, but Collignon was the first to study this factor in the living. The two height indices are obtained by comparing the total height of the head measured from the vertex to the centre of the ear-hole with—(1) the length of the head, and (2) its greatest breadth, each of these two diameters being taken as 100.

The indices are classified as follows :—¹

	Height-length Index.	Height-breadth Index.
Platycephals - - - -	- 67	- 83
Mesocephals - - - -	67 - 70	83 - 85
Hypsicephals - - - -	70 +	85 +

A really high skull, if it is very broad, may appear relatively low, or a low, but very narrow head, may appear decidedly hypsicephalic. Hence the necessity to consider first the cephalic index and thereby to recognise the normal and harmonic fluctuations of the inverse variations of these two vertical indices.

¹ These indices are taken from a subsequent memoir by Dr. Collignon. (*Mém. Soc. d'Anthrop.* Paris, i. (3^e sér.), 1895, pp. 94, 95.)

Dr. Collignon has plotted the distribution of these indices for the Department of Dordogne alone. We have seen that the northern cantons are what he termed dolichocephalic, and the southern are brachycephalic. The length-height index of the former varies between 65 to 68, and of the latter from 70 to 72. Taking the mean at 66 and 70 respectively, it follows that the dolichocephals are platycephalic and the brachycephals hypsicephalic; but this platycephaly is a true flattening of the skull, and is not merely due to a lengthening of the cranium, as it is not the most dolichocephalic cantons that are the most platycephalic.



FIG. 5. THE DISTRIBUTION OF THE HEIGHT-LENGTH INDEX IN DORDOGNE.



FIG. 6. THE DISTRIBUTION OF THE HEIGHT-BREADTH INDEX IN DORDOGNE.

70 +	Hypsicephalic (shaded)	85 +
67 - 70	Mesocephalic (blank)	83 - 85
- 67	Platycephalic (cross-hatched)	81 - 83

The oblique band enclosed with a thick line corresponds to the division between the dolichocephals and brachycephals (see fig. 2).

On the other hand, all the brachycephalic cantons have a height-breadth index of from 81 to 84, that is they are, or appear to be, platycephalic and mesocephalic, but their mean is mesocephalic.

The mixed race who inhabit the zone between the brachycephals and dolichocephals (cephalic index 80-82) is also intermediate with a height-breadth index of 83-85, but the dolichocephals fall into two groups, the one with indices from 85 to 87 are hypsicephalic, the others, like the brachycephals, are mesocephalic and platycephalic.

Thus the platycephaly of the valley of the Isle is established.

The brachycephals are only false platycephals owing to an exaggeration of the transverse diameter.

Without going into further details, we may now make an attempt to unravel the ethnology of these five Departments. Taking the three characters of cephalic index, colour and stature, we can distinguish : short and dark or tall and fair brachycephals ; fair tall dolichocephals and dark dolichocephals.

The brachycephals occupy all the region south of the rivers Dordogne and Vézère, the whole of the Department of Corrèze and the east of that of Creuse. The brown brachycephalic type extends to the mountainous region of Auvergne, to the East of France and to the South of Germany. This race of short, dark brachycephals is a well-marked type which has received several names. Dr. Collignon, for want of a better term, adopts Broca's designation of Celts, as the founder of French Anthropology considered that these were essentially the Celtæ of Cæsar. They are often called Auvergnats. The tall fair variety is due to a crossing of this type with the fair race. A similar racial mixture occurs in Lorraine.

The fair dolichocephals inhabit the upper valley of the Cher ; the neighbourhood of Limoges, whence they spread to the south following the plateaux that separate the valleys of the Isle and of the Dordogne ; and also the north of Charente, Angoulême, and in general along the very ancient route between Paris and Bordeaux. These are the modified descendants of the tall, fair, dolichocephalic race of North Europe. Dr. Collignon speaks of it as the Hallstadt race.

The brown dolichocephals require a further analysis.

1. A type can be distinguished which is characterised by its relative platycephaly, the extreme broadening of the face, a prominent chin, low orbits and by the dark colour of the skin and hair. As it is usual in Europe to correlate a long narrow face with a long head, and a short, broad face with a rounded head, the association, as in this case, of a long

head with a broad face forms what is termed a hisharmony. In the fair dolichocephals, on the other hand, the head is high, the face narrow, the chin moderately prominent, the orbits normal, the skin, hair and eyes fair. It is obvious that these two races are entirely distinct.

2. A narrow-faced dolichocephal with a high head can be distinguished, but Dr. Collignon believes that it is a cross between two races, the brown and the fair dolichocephals. This is a very favourable combination, and gives rise to a beautiful variety of man.

3. A rare but recognisable type, with an extraordinarily narrow and elongated face, a retreating forehead, projecting jaws and retreating chin, the concave nose is so broad as to be nearly platyrrhine, the hair and skin are dark.

Putting the second of these two varieties out of count, there only remain the brown dolichocephal with a dysharmonic face, and that with a retreating chin. They both live in the basin of the Isle and its affluents, as much in Charente as in Dordogne.

From numerous other investigations we know that the Neolithic dolichocephals of Western and Southern Europe were a slight people with brown hair. They constitute the Mediterranean race of Sergi, the western branch being generally termed Iberians. The ancient cave-men of France belonged to the same race; by comparing certain indices of these with the first group of our brown dolichocephals we find a remarkable correspondence:—

	Cephalic index.	Height-length index.	Height-breadth index.
Caverne de l'homme mort -	Dolicho.	Platyceph.	Mesoceph.
Old man of Cro-Magnon -	„	„	Platy.
Recent Dordogne - -	„	„	Platy. & Meso.

Further, the Cro-Magnon man had a dysharmonic face, this is also characteristic of the Neolithic dolichocephal of Laugerie, and it survives in their descendants in the valley of the Isle.

The remaining brown dolichocephalic type, with its low-

typed, long, narrow, prognathous face, is considered by Dr. Collignon to be the far-removed descendants of the Quaternary race of Canstadt and Spy. The same type has been recognised by him in Tunis among the Berbers of Djerid (Collignon's "*type du Gétule*") as well as in Dordogne and in the south of Charente, that is to say, in places still occupied by the descendants of the race of Cro-Magnon. It might be expected that the very ancient race of Canstadt, and the later race of Cro-Magnon, were together beaten back by the great pre-historic invasions of Western Europe.

A few words will suffice to trace the pre-historic settlements and racial movements that have occurred in this district.

The earliest inhabitants were probably the people with retreating chins. According to the opinion of Dr. Collignon these were kinsmen to Palæolithic man. At the present day, as is only to be expected, this type is very rarely met with in anything like purity, and it is very difficult to isolate it statistically.

The whole west of Europe was later occupied by the brown dolichocephals, the Iberian branch of the great Mediterranean race, of which the Cro-Magnon man was a variety. They buried their dead in the caves of the valleys of the Vézère, Isle and Dronne. Judging from their art, they were a skilful people and not devoid of culture :—

Later he pictured an aurochs—later he pictured a bear—

Pictured the sabre-toothed tiger dragging a man to his lair—

Pictured the mountainous mammoth, hairy, abhorrent, alone—

Out of the love that he bore them, scribing them clearly on bone.

There, protected in their barren, rocky valleys, weathering the storm of race conflict, unsubmerged by waves of race migration, still survive the children of early neolithic man.

Also in neolithic times a short, dark, brachycephalic folk came into France from the east by two routes flowing north and south of the Alps. The invasion followed the left bank of the Danube, entered the valley of the Rhine, and later

spread into France through the pass of Belfort and by the lower Moselle. A second, probably later and less important, invasion crossed the river to reach Upper Italy and Switzerland, and thence gained the valley of the Rhone. Thus their migration has been from east to west.

When the invasions came of the tall, fair dolichocephals : Kymri, Gauls, Cimbrians, Burgundians, Visigoths, Franks, etc., they more particularly followed a course parallel to the North Sea. From an ill-determined point to the north-east or north, they advanced invariably along the plains, probably on account of the chariots which they always brought with them. After having covered the plains of North Germany, where since then their descendants have always lived, and which became a second centre for emigrations, they passed to the north of the Black Forest to scatter upon the Netherlands and Flanders, the valley of the Seine and that of the Rhine. Thence their swarms were divided by the central plateau of France, one stream being diverted into Italy, the other into Spain and thence to North Africa.

The Roman conquest scarcely, if at all, affected the population of these five Departments, and it is more than certain that since then no foreign element has produced any result that can be traced, for all the Barbarians, as well as the English, belonged to the fair race.

In a subsequent memoir on the Anthropology of the South-West of France (*Mém. Soc. d'Anthrop.*, Paris, i., 3^e sér., 4^e fascic., 1895) Dr. Collignon sums up his conclusions as follows :—

Such is, after an examination of anatomical characters, the distribution of the races in the south-west of our country. Is it possible to draw therefrom reliable indications of what it was formerly? Regarding this we may lay down this rule. When a race is well seated in a region, fixed to the soil by agriculture, acclimatised by natural selection and sufficiently dense, it opposes; for the most precise observations confirm it, an enormous resistance to absorption by the new comers, whoever they may be.

The most striking example of this stability of seated races, of this force of inertia which renders them victorious,

is certainly presented to us by Egypt. The modern Fellah differs in nowise from his ancestors, several millenniums ago who lived at the times of Tothmes and Rhameses, although, according to the calculations of M. Hamy, slavery had introduced upon the borders of the Nile more than 20 millions of negroes. These, in a climate which at first sight would be favourable to their acclimatisation, were not able to perpetuate their race, neither directly nor indirectly, that is to say, by crossing. All the more reason, one may say, that the same can be said of the historic conquerors of this unfortunate country, from the Hyskos and the Persians up to the Turks and the latest comers, the English. The waves of foreign blood that have spread over Egypt have disappeared never to return.

The reasons are many. If the aboriginal race is more numerous than its invaders, and this is nearly always the case, it can not be entirely destroyed, whatever be the slaughter which accompanies the conquest; the women and the children are preserved. The importance of the subsequent crossings can not then, at the maximum, attain more than one-third. The stable condition that follows puts then *ipso facto* the new comer in a minority from the commencement of the conquest, the work of selection by acclimatisation does the rest. It is a matter of a few generations.

The only case where the occupation can be definitive is that of an invasion by a very superior race emigrating with women and children to a region peopled by nomads or true savages,—such as the occupation of the United States or of Australia by the Europeans. In Canada, despite the political occupation and the incessant arrival of emigrants of their own blood, the English are absolutely balanced by the old French element, who were masters of the soil before their arrival.

But the presence of woman at the time of a conquest, if she is indispensable to a real and definitive colonisation, since alone it ensures the perpetuity of pure descendants, is not, however, sufficient. Except in a savage country, the women of the conquering party would always be in a

minority. Even in the case where restrictive laws would assure to their progeny particular privileges, making a kind of aristocracy, it could never happen that there would be only two strata of the population, a victorious aristocracy superimposed upon a conquered democracy. We know the fate of all aristocracies. Their grandeur is their ruin; they survive thanks only to foreign relays, and on an average disappear in three or four centuries. One cannot say "*Væ victis*," but "*Væ victoribus*," everything comes to him who waits.

The Romans did not systematically depopulate Gaul, her submission satisfied them; the distribution of races at the time of the Roman peace did not undergo other changes than those which could operate quite locally, the deporting of a too obstreperous people or colonising by veterans. The Barbarians passed like a torrent, they destroyed much, but they have not made in their campaigns a true colonisation "*ense et aratro*" of Marshall Bugeaud. The sword sufficed to assure their domination; to the vanquished—work. They have disappeared, except perhaps in the towns where they crossed with the Gallo-Roman middle class, after having preserved the forms or the imperial administration, for want of knowing and of being able to do better. The Arabs traversed the country, but to immediately disappear. It results, once more let it be repeated, that the present distribution of races should faithfully represent to us their ancient distribution, except in places where special economic conditions have been slowly modified, but in a constant manner, by foreign influences.

A. C. HADDON.

THE INFLUENCE OF OXFORD ON THE HISTORY OF GEOLOGY.¹

ONE of the most immediate effects produced by contemplation of the world around us is that of pleasure in its abounding beauty. Whether we wander through the smiling country which surrounds this University, or climb the snowy peaks of the Alps, or visit the sunlit islands of the Tropics, wherever we turn, the feeling expressed by a German poet will spontaneously arise: "Oh! Wunder schön ist Gottes Erde, Und Schön auf ihr ein Mensch zu sein"—Beautiful is God's Earth and good it is to be a Man thereon.

This æsthetic delight, while it may sometimes suffice for the poet, is succeeded generally by a desire for closer acquaintance; a certain divine curiosity implanted in the breast of man leads him to search into the inner mysteries of Nature, and to explore the causes of the wonderful phenomena which surround him. He begins to examine and compare, to analyse the complex into its elements, and to build up again the elements into a new and intellectual cosmos. Thus he gains a new pleasure from contemplation of the world, and labouring in the pleasant work of investigation he experiences the joy of discovery.

The pleasures of sport are generally supposed to be those to which the soul of the Englishman is most deeply responsive. Thus the famous Darwin writing of his youth remarks: "The autumns were devoted to shooting . . . my zeal was so great that I used to place my shooting boots open by the bed-side when I went to bed, so as not to lose half a minute in putting them on in the morning. . . . How I did enjoy shooting!" But a little later, and he writes: "During the first two years my old passion for shooting survived in nearly full force, and I shot myself all the birds and animals for my collection; but gradually I gave up my

¹ An Inaugural Lecture delivered before the University of Oxford, on 19th October, 1897.

gun more and more, and finally altogether, to my servant, as shooting interfered with my work, more especially with making out the geological structure of a country. I discovered, though insensibly and unconsciously, that the pleasure of observing and reasoning was a much higher one than that of skill and sport."

This fragment of personal history seems to me even more impressive than the glowing words of Thierry, who although himself an invalid, wrote "Il y a au monde quelque chose qui vaut mieux que les jouissances matérielles, mieux que la fortune, mieux que la santé elle même, c'est le dévouement à la science!"

So much I have ventured to say in eulogy of all Science, of my own subject I would only add that it enjoys what is supposed to be a thoroughly English characteristic, it is fond of the open air; its best work can only be accomplished in the open air, it is there that its greatest triumphs have been won, and must continue to be won.

Many great discoveries have been made in Science by observers who did not consciously set out with a predetermination to discover something, but whose imagination was fired by some object or occurrence rather out of the ordinary course of Nature; as Bacon has observed: "It is probable that Prometheus, when he first struck the flint, must rather have marvelled at the spark than expected it". With the advance of Science we become less open to surprises, and the number of discoveries due to some happy chance constantly tends to diminish; such, on the other hand, are peculiarly characteristic of Science in its infancy. It would appear that the strange phenomenon which first directed attention to the problems of geology was the occurrence of curious stones, such as are now termed fossils, that, having a likeness to the hard parts of animals and plants, are yet found, to the surprise of the observer, lying embedded in the substance of rocks which form the interior of a country often at a great distance from the sea, and sometimes at a great elevation above it, as in the case of fossils in the Himalayas, which are obtained from that lofty mountain chain at a height of 16,000 feet.

No doubt the nature of fossils had awakened a lively interest in very early times. In Babylonia, that ancient mother of Arts and Sciences, many different kinds of fossils must have been familiar objects to the workmen who dug clay for bricks, or hewed stone out of the quarry; they were probably seen and pondered over by Chaldean philosophers, whose speculations concerning them were afterwards embodied in a mythical cosmogeny.

In ancient Greece they were well known and understood; but in Christian Europe, up to the sixteenth century, the civilised world was occupied with problems more important than the study of fossils; and even when in the revival of learning Fracastoro, Leonardo da Vinci, and Pallisy the potter, uttered true words concerning them, these were little heeded, and it was not till after the middle of the seventeenth century that fossils commenced to arouse that serious attention which with increasing earnestness has continued to be given to them ever since. It is in two little books, one entitled *Canis Carchariæ Dissectum Caput*, and the other *De Solido intra Solidum Naturaliter Contento*, published in 1669, that the germ of modern geology is to be found. They were written by the famous Steno, the true founder and father of our science. Steno was by birth a Dane, who occupied for a time the Chair of Anatomy at Padua. Dwelling thus not far from the shores of the Adriatic, he was able to make studies in marine zoology, and of these the most important was his dissection of a shark's head. Steno paid particular attention to its jaws and teeth, which are well worthy of study, as being probably the sharpest cutting instruments naturally produced. But these put him in mind of certain "glossopetræ" which are dug out of the ground in Malta, and on making a careful comparison of the fossil with the recent teeth he convinced himself of their precise similarity. But according to Steno's logic, nothing but a shark could make shark's teeth, and he consequently concluded that the ancient glossopetræ had once belonged to the mouth of a shark, and that the mouth opened into a shark's body behind it. You cannot assert, he remarks, that Nature will make the hand of a man with-

out the man himself, and the argument is just as true of sharks' teeth. Lest the great numbers in which the glosso-petræ occur should be thought a difficulty—they were carried away from Malta in bushels—he points out that every shark's jaw contains sixty teeth in good working order, and these as they wear out are continually replaced by fresh ones; further, he adds, sharks swim about in schools, so that a great many teeth will generally be found bristling about in the same place at the same time. Since sharks are very voracious creatures, the multitude of glossopetræ implies the existence not only of sharks, but of a whole world of other animals, including those which live on cockles and other shell-fish. That we next find Steno giving serious attention to the study of cockle-shells is only therefore what we might expect. He patiently traced the manner of their growth, and ascertained, with a degree of accuracy remarkable for those times, the minute details of their structure. From the living cockle he turned to the fossil shells, and by a careful comparison showed how they agree, feature by feature, both in form and more particularly in structure, with their modern representatives. This structure, Steno insists, is the direct result of the manner in which the shell is produced by the animal, and he concludes that the fossil, like the recent cockle-shell, once contained a living cockle inside it.

Having thus, by close observation and strict logic, breathed life afresh into these fossil remains, Steno proceeded to provide them with an environment: they were bathed, he says, with an ambient fluid, which was none other than sea-water; hence it follows that the lands in which such fossils occur, were once submerged beneath the sea. This conclusion, he pointed out, is in complete harmony with the nature of the material of which the land consists, and in which the fossils lie embedded, for it resembles in the most striking manner the ooze of the sea-floor. In support of his argument Steno called attention to many interesting points of detail, such as the discovery of a fossil pearl-oyster, with a pearl still sticking to it: of fossil oysters perforated by galleries such as are produced by boring

worms ; and of another fossil shell, covered with barnacles, which were adherent to its worn surface, a fact from which he drew several ingenious conclusions.

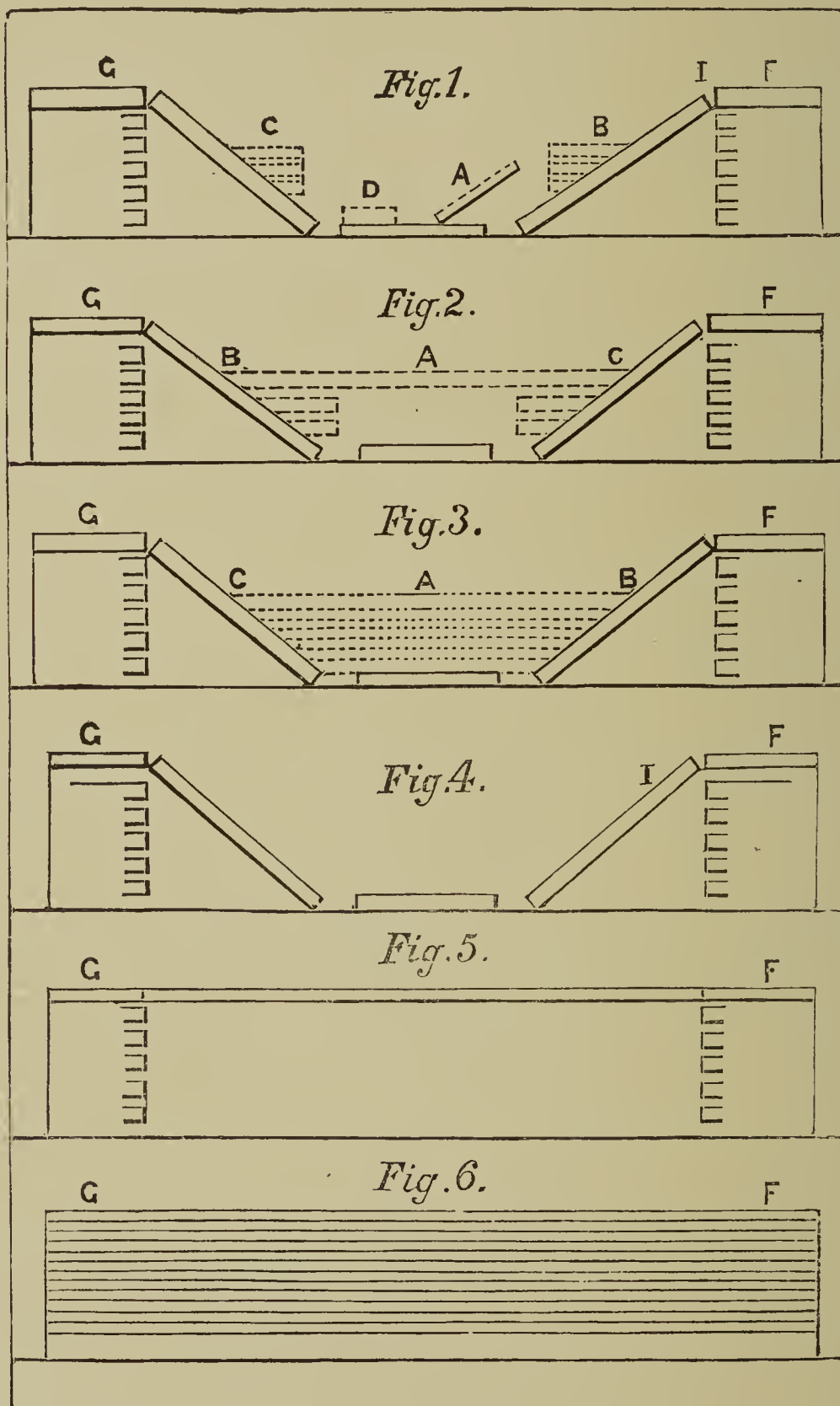
Returning to the sediments in which fossils lie buried, Steno pointed out that they are distributed in beds or strata, which are frequently horizontal ; and he concluded that the edges of each bed corresponded originally to the margin of some sea, and its lower surface to the sea-floor. In a series of beds all except the lowest were originally contained between two planes parallel to the horizon. In these statements there is contained implicitly the modern doctrine of super-position, *i.e.* that the order in which beds succeed one another vertically in space is the order in which they have been deposited, or the order of their succession in time. This is explicitly stated by Steno in another paragraph “at what time there was formed any Bed, the matter incumbent on it was all fluid, and by consequence, when the lowest Bed was laid, none of the upper Beds was extant”.

Steno however pursued his inquiry far beyond this stage ; he proceeded to point out that some strata occupy an inclined or even vertical position, and he rightly inferred that these must have been tilted out of the horizontal by some natural disturbance subsequent to their formation.

The crowning triumph of Steno’s achievement lay in the application which he made of these conclusions to the explanation of the structure of Tuscany.

The structure of Tuscany as Steno conceived it is shown in the following diagram, copied from his work : *De Solido intra Solidum Naturaliter Contento*. The mountains are for the most part composed of horizontal beds, C, F, but dislocated and sloping towards the low ground, on their flanks. On these fundamental rocks others of a later date, C, A, B, rest discordantly ; and thus it appears that Steno was familiar with the important phenomenon now termed an unconformity. Now since the strata were originally deposited as continuous horizontal layers, it follows that the rocks C, A, B at one time extended in an unbroken sheet between the flanks of the surrounding mountains, as shown in Fig 2. Further since the bed B, A, C could not have

been self-supporting, but must have rested on older sediments the gap below it must have been filled up with a succession of horizontal deposits as shown in Fig. 3. This state of things again was not original, there was a time when these sediments did not exist, and the Tuscan district



appeared as in Fig. 4. Once more, the argument which applied to the latest rocks, holds equally for the older ones G, F, so that we must restore these, first as represented in Fig. 5, and next as in Fig. 6. Beyond the last stage, when Tuscany lay inchoate, a mass of sediments beneath a primæval sea, we can only take one step farther back into the

abyss of time, and this brings us to a period, antecedent to the deposition of any sediment whatever, and to a state of Tuscany which Steno does not venture to represent by a diagram.

Steno did not regard the explanation, just set forth, as restricted in its application to Tuscany; but distinctly affirmed, from his knowledge of other countries, that similar reasoning would hold for every part of the world, where stratified rocks occur.

The first to logically demonstrate the true nature of fossils; the first to recognise the orderly sequence of deposits; the first to perceive their occasional interruption by those discordances, which we now term unconformities; the first to employ geological structure to arrive at geological history and to inform us of the revolutions through which our globe has passed; said we not truly that this man was the father and founder of our science?

In attempting to carry his explanations farther and to push them deeper, Steno was less happy: his brave imagination, successful so far, was now to suffer from the restraints imposed upon it by a belief in the brevity of the world's existence. The stage which immediately preceded that represented by Steno's sixth diagram stood before his mind as the beginning of created times, and all the subsequent events of the earth's history had to be compressed into a poor 6000 years! Without a more generous allowance of time no genius, however gifted, could hope to trace the slowly pacing processes of nature; and the fame of Steno would undoubtedly have burned brighter had he desisted from the attempt and refrained from an endeavour to reconcile the irreconcilable.

Contemporary with Steno was the celebrated Dr. Plot of this University, whose great work on *The Natural History of Oxfordshire* appeared in 1677, eight years after the publication of Steno's *De Solido*, etc. This was 220 years ago, in the middle of the reign of Charles II., about fifty years after the Fellows of Wadham College and their friends met together to bring into existence the Royal Society, for which the good king did so much by judi-

ciously letting it alone; modern chemistry had but just sprung into being under the influence of Robert Boyle, and Newton's *Principia* had not yet enlightened the world.

The History of Oxfordshire is a very interesting work, full of queer old-world information, quaint digressions and pleasant meanderings, winding gently on each side of the main current of discourse. It contains a map of Oxfordshire curiously bordered round with the arms of the resident nobility and gentry, and each of its excellent plates of illustrations bears a dedication in the corner to some noble patron, with the emblazonment of his arms.

Dr. Plot was evidently acquainted with the views of Steno, but he did not share them; on the contrary, he met them with a strenuous opposition, in which he displayed great resources of learning and dialectic skill. A whole chapter of the *History* is devoted to the description of the fossils or, as they are there termed, the "Formed Stones" of the county. It is illustrated by engravings which, for truthfulness, leave little to be desired, so that, even at this distance of time, there is little difficulty in recognising and identifying the different genera or even species that they represent.

The stones to which Dr. Plot first directs attention are those that stand in some connection with the heavenly bodies, such as the Sun-stone, of which, however, there are none in Oxfordshire; the Moon-stone or Selenites; the Asteriæ or Star-stones, which are evidently the joints of the stem of a fossil, now called *Extracrinus briareus*; the Astroites, bodies of irregular form, but adorned with constellations of stars; these are clearly corals, such as are now named *Isastrea* and *Thamnastrea*; and finally such stones as are supposed "by the *vulgar* at least" to be "generated in the *Clouds* and discharged thence in the times of *Thunder* and *violen Showers*"; such are Belemnites, and Brontiaæ and Ombriaæ: in the two latter we now recognise several kinds of Sea-urchins. The author then passes to "The *Stones* that concern the *Watery Kingdom*"; some of these are spars or minerals; others are true fossils such as a "Strombites," which is compared to the living "Concha Tridacna," so

called, the author quaintly, though erroneously, observes, "because they made three Mouthfuls apiece," and he ingenuously adds that were "the Strombites not a Stone I must pronounce it the Same," *i.e.*, as the living *Tridacna*. A "Conchites" of the kind we now term "*Rhynchonella*" is introduced to us, and in a digression we are informed that specimens of this "made red hot and put into drink are accounted a present Remedy for a Stitch".

There is also a *Pecten*, which the author compares with the *Pecten asper* of Aldrovandus, and an Oyster (evidently *Ostrea dilatata*) of which it is remarked "I could easily have assented that these . . . might once indeed have been Shell-fish, but that (just as with the Escallops) we only find the *protuberant parts* of the Shells [convex valves] and never any of the *flat ones*". We then come to a fossil, now known as *Cidaris*, which is spoken of as "a curiously embroidered Stone, much resembling the petrified *Riccio Marino*, or Sea-Urchin of *Imperatus*," also known to old authors as *Mamelles de Saint Paul* and as *Ova Anguina* "because from the *Basis* there issue as it were five *Tails of Serpents*, waved and attenuated towards the Upper part of the *Stones*". These "Old Authors" regarded it as "engendered from the *Salivation* and *Slime* of *Snakes*, and cast into the Air by the Force of their *Sibilations*: where if taken, has Effects as wonderful as its Generation, and therefore of great Esteem amongst the *French Druids*". "But," concludes our excellent author, "I care not to spend my time in *Romance* and therefore proceed," and so passes to the *Cornua Ammonis* or *Ophiomorphites*, our *Ammonites*. Some of these are found "about Adderbury, about two Miles from *Banbury*, but . . . that the Town has not its Name from these Stones (as Mr. Ray thinks) I dare confidently avouch, *Adderbury* being only the vulgar Name: for in the *Court-Rolls* of *New College* . . . it is written *Eabberbury*, perhaps from St. *Ebba*, the tutelar Saint of the *Church*".

The "Formed Stones" having been sufficiently described, Dr. Plot proceeds to a discussion of "The great *Question* now so much controverted in the World: Whether

the Stones we now find in the form of *Shell-fish* be *Lapides sui generis*, naturally produced by some extraordinary *plastic virtue*, latent in the Earth or Quarries where they are found? Or whether they rather owe their Form and Figuration to the *Shells* of the *Fishes* they represent, brought to the *places* where they are now found by a *Deluge*, *Earthquake* or some other such means, and then being filled with *Mud*, *Clay*, and petrifying *Juices*, have in tract of time been turned into *Stones*, as we now find them, still retaining the same Shape in the whole, with the same *Lineations*, *Sutures*, *Eminences*, *Cavities*, *Orifices*, *Points* that they had whilst they were *Shells*?" "In the handling thereof" the author modestly disclaims any intention of arriving at a "peremptory *Decision*" and invites merely to "a friendly *Debate*". Let no one be beguiled, however, by these fair words, which are but the bow of a champion on entering the arena. It is true the debate proceeds smoothly enough, but it is also conducted according to all the rules of fence and with the art of a master. Steno's weakest point was the deluge, which he had to bring over Tuscany in his final explanation of geologic changes. Plot, with unerring instinct, makes straight for this. He considers first the difficult question of the means by which these fossils, if they were originally parts of living animals, could have been transported from the sea to the interior of the country: deluges had been suggested, but with deluges, whether Noachian, Ogygian, Deucalionian or purely local or national floods, Plot will have no dealings, alleging very sufficient reasons for regarding all deluges as unfitted, by their very nature, for the effects required; while as to earthquakes, he remarks, that to suppose "the Mountains (where such Stones as most resemble them [shell-fish] are now found) were heretofore low places and since raised by *Earthquakes*: [is] a thing by no means to be believed of our *Northern* Parts, where the *Earthquakes* we have at any time are so inconsiderable that they scarce sometimes are perceived, much less affrighten us; unless we shall groundlessly grant, that in the infancy of the *World*, the *Earth* suffered more concussions, and consequently more mutations in its *Super-*

ficies, than it has done ever since the Records of Time":—an argument that cannot but appeal to every orthodox Uniformitarian.

Having thus shown an antecedent improbability against the view that fossils are the remains of organisms, he proceeds to attack the enemy in his stronghold. It is affirmed, Dr. Plot remarks, that "these *Formed Stones* are many of them in all respects, like the living *Shell-fish*; thus says Boccone, the *Herrison's Spatagi* of *Stone*, the *Cornua Ammonis* or *Nautili Lapides* have the very Marks, Characters, Eminences, Cavities and all other parts alike, with the true living *Nautili*, and *Herrison's Spatagi*, . . . which proves, says he, *the Body changed to have been the very same thing with that which is living*. But I must tell him, it does but very weakly, all Arguments drawn *a similitudine*, being the most inefficacious of all others, such rather illustrating than proving, rather perswading than compelling an *Adversary's* Assent. For how many hundred things are there in the *World*, that have some Resemblance of one Another, which no Body will offer to think were ever the *same*, and particularly among some other *Formed Stones* hereafter to be mentioned. Such are the *Stones Otites*, or *Auriculares*, several sorts of *Cardites*, which though they as exactly resemble those parts of *Men*, from whence they have their *Names* as any *Conchites* or *Echinites* do those *Shell-fish*: yet no *Man* that I ever heard of, so much as dreamed that these were ever the real parts of *Men*, in process of time thus turned into *Stone*. As well might we say that our *Kettering Stone* in *Northamptonshire*, here in *England*, was once nothing else but the spawn of Lobsters: than which that I know of, there is nothing more like." If I might express an opinion, I should say, that as it stands, this is a very excellent argument: but it would have been open to an opponent to reply that it is vitiated by one serious mis-statement: the simulacra of human organs which are referred to as *Otites* and *Cardites* do *not* as exactly resemble those organs, as the *Conchites* and *Echinites* do those *shell-fish*: in the latter case the resemblances are far more numerous and precise, and are, if

anything, more marked in the internal structure than the external form ; so, that even when agreement in form fails, similarity in structure which is all important remains. This cannot be said of the Otites and Cardites.

The argument is then pushed a step farther by pointing out that the resemblance between organism and formed stones is frequently not so close as is asserted : thus the *Cornua Ammonis* had been likened to the shells of *Nautilus* from which they certainly differ in important particulars ; but in addition to these minor points of difference Dr. Plot triumphantly cites instances in which they had been observed with “heads”. “‘Vidimus enim lapidem hinc delatum surpentis in spiram revoluti effigie, cujus caput in circumferentia prominuit, extrema cauda centrum occupante,’ are the very words of Mr. *Cambden*”.

Although this instance is an unfortunate one, since the “heads” were no doubt the work of some practical joker or fraudulent fossil finder, yet the argument will not be greatly affected, since it is admitted that the fossil forms often differ markedly from their living representatives. The explanation of these differences had been arrived at long before by Palissy, and is suggested by a writer, bearing the illustrious name of Lister, who says truly that these fossils must be either “terrigenous, or if otherwise, the animals they so exactly represent have become extinct”. Referring to this Dr. Plot remarks :—“If it be said, that possibly these Species may be now lost, I shall leave it to the *Reader* to judge, whether it be likely that *Providence*, which took so much care to secure the works of the *Creation* in *Noah's Flood*, should either then, or since, have been so unmindful of some *Shell-fish* (and of no other *Animals*) as to suffer any one *Species* to be lost”.

Since this was written we have learnt to accept the doctrine of the extinction of species ; it has become a common-place amongst the educated, and has passed into the language of our poets :—

So careful of the type she seems ! But no !
From scarped cliff and quarried stone
She cries a thousand types are gone !
I care for nothing ; all shall go.

After his exposition of the weak points in Steno's argument Dr. Plot proceeds to consider the alternative view, that the formed stones are the result of some "plastic force"; and as later writers have made merry over this expression, it will be merest justice to explain what Dr. Plot meant by it. Whatever the plastic force of Theophrastus may have been, that of Dr. Plot was certainly what we now recognise as crystallisation. His words are: "There is no other Principle that we yet know of naturally shooting into *Figures*, each peculiar to their own kind, but *Salts*, thus *Nitre* always shoots into *Pyramids*, salt *Marine* into *Cubes*, *Alum* into *Octo*, and *Sal Ammoniac* into *Hexahedrons*, and other mixt *Salts* into as mixt *Figures*.

"Of these spontaneous inclinations of *Salts* each peculiar to its *Kind*, we have further evidence in the *Chymical Anatomy of Animals*, particularly in the *Volatile Salt* of *Harts-horn*, which in the Beginning of its Ascent is always seen branched in the Head of the *Cucurbit*, like the Natural *Horn*. And we are told by the very ingenious and learned *Sidleyian Professor* here in *Oxon*, That the *Salt* of *Vipers* ascends in like manner, and shoots into *Shapes* somewhat like those of *Animals*, placed orderly in the *Glass*. Thus in *Congelations*, which are all wrought by adventitious *Salts*, we frequently find curious *Ramifications*, as on Glass-windows in Winter, and the figured Flakes of *Snow*, of which Mr. *Hook* observed above an Hundred several sorts, yet all of them branched as we paint *Stars*, with six principal *Radii* of equal Length, Shape and Make, issuing from a *Center*, where they all joined in *Angles* of 60 *Degrees*.

"What *Salt* it should be that gives this *Figure*, though it be hard to determine, yet certainly it must not be a much different *one* from *that* which gives Form to our *Astroites* and *Asteriæ*, whereof, though the latter have but five *Points*, and therefore making *Angles* where they are joined at the *Center* of 72 *Degrees*: yet the *Astroites* both in *mezzo Rilievo* and *Intagli* have many more.

"Perhaps there may be something of an *Antimonial Salt* that may determine Bodies to this *Starry Figure*, as no Question it does in the *Regulus*, and the *Caput mortuum* of

the *Cinnabar* of *Antimony*. To such a *Salt* may also be referred our *Brontiaë* or *Ombriaë*, and all the *Echinites*, some whereof are plainly, *all* in some measure *stellated* at the Top.

“The *Belemnites*, which are all *striated* from a *Center*, yet in the whole affect a *Pyramidal* Form, seem to have somewhat also of an *Antimonial*, but a more prevalent Quantity of a *Nitrous Salt*.

“The *Conchites*, *Pectinites* and *Ostracites*, whether transversely *striated*, or from the *Commissures* to the *Rim*, seem to owe their origin to *Urinous Salts*, which start likewise from a *Center* (as suppose from the Hinges of these *Stones*), but generally are most extended to one side, as may be seen in the branched Figure formed on the Surface of *Urine* by freezing, as in Mr. *Hook's Micrography*, whose *Striæ* not obtaining much above the *Quadrant* of a *Circle*, whatever other Differences they may be, in this respect at least is agreeable to our *Stones*.

“To which add the *Ophiomorphit's* or *Cornua Ammonis*, most probably formed either by the *Salts* shooting different ways, which by thwarting one another make a *Helical* Figure, just as two opposite Winds or Waters make a *Turbo*, or else by some simple, yet unknown *Salt*, that affects such a Figure: perhaps the Stems and Branchings bended in a most excellent and regular Order, like the Ribs of some of our *Ophiomorphit's*, observed by Mr. *Hook* in *Regulus Martis Stellaris*, might not a little conduce to the clearing of this Matter.”

The difficulties which beset this explanation are duly considered and disposed of; the last of them is so quaintly treated that I cannot forego the pleasure of quoting it. “That it seems quite contrary to the infinite *Prudence* of *Nature*, which is observable in all its Works and Productions, to design every *thing* to a determinate *end*, and for the attaining of that *End*, makes use of such ways as are (as far as the Knowledge of *Man* has yet been able to reach) altogether consonant and agreeable to *Man's* Reason, and of no way or means that doth contradict, or is contrary to *Human Ratiocination*: Whence it has been a general Ob-

servation and *Maxim*, that *Nature doth nothing in vain*. It seems, I say, contrary to that great Wisdom of *Nature*, that these prettily shaped Bodies should have all those curious Figures and Contrivances (which many of them are adorned and contrived with) generated or wrought by a *plastick Virtue*, for no other higher End than only to exhibit a *Form*. To which I answer, that Nature herein acts neither contrary to her own *Prudence*, human *Ratio-cination* or in *vain*, it being the Wisdom and Goodness of the *Supreme Nature* (by the Schoolmen called *Naturans*) that governs and directs the *Natura naturata* here below, to beautify the World with these Varieties, which I take to be the End of such Productions as of most *Flowers*, such as *Tulips*, *Anemonies*, etc. of which we know as little Use as of *Formed Stones*. Nay, perhaps there may proportionately Number for Number be as many of *them* of *Medicinal* or other *Use*, such as *Selenites*, *Belemnites*, *Conchites*, *Lapis Judiacus*, etc., as there are of Plants: so that unless we may say also (which I guess no Body will) that these are produced contrary to the great Wisdom of Nature, we must not of *Stones*."

There were however certain fossils, known to Dr. Plot, the organic nature of which he was ready to admit, for they possessed not only the outward form of bones, but exhibited, though turned to stone, a characteristic bony structure. One of these was a thigh bone, of what nature it would from the description be difficult to say; it was found in the parish of Cornwell and is thus considered by the author: "It remains", he says, "that it must have belonged to some greater *Animal* than either an *Ox* or *Horse*: and if so (say almost all other *Authors* in like *Case*) in all probability it must have been the *Bone* of some *Elephant*, brought hither during the Government of the *Romans* in *Britain*: but this Opinion too lies under so great Difficulties, that it can hardly be admitted; which are briefly these:—None of the *Roman Authors*, who elsewhere are large enough in describing the *Elephant's* Behaviour in *Fight*, and how terrible they were to some of the *Trans-Alpine* Nations, mention any such Matter in any of their *Expeditions* into

Britain. . . . One there was 'tis true sent hither as a Present by St. *Lewis IX.*, King of France to King *Henry III.* *Anno 1255*, which, says *Matthew Paris*, was the first seen this side of the *Alps*, and perhaps there may have been two or three brought for Show hither since: but whether it be likely any of these should have been buried at *Cornwell* let the *Reader* judge."

A point is made of the fact that tusks had not been found associated with the supposed elephant's bones, a piece of negative evidence, the worthlessness of which is shown by the subsequent discovery of such tusks.

Instances are then given of the exhumation of similar bones from churchyards. "Since the great Conflagration of *London*, *Anno 1666*, upon the pulling down of St. *Mary Wool-Church*, and making the site of it into a *Market-place* there was found a *Thigh-bone* (supposed to be of a *Woman*) which was to be seen at the *King's Head Tavern* at *Greenwich* in *Kent*, much bigger and longer than ours of *Stone* could in proportion be, had it been entire." Two other similar bones were dug up in the parish churchyard of *Merton Valence*. "Now," exclaims our author in triumph, "how *Elephants* should come to be buried in *Churches*, is a Question not easily answered, except we will run to so groundless a shift, as to say, that possibly the *Elephants* might be there buried before *Christianity* florished in *Britain* and that these Churches were afterwards casually built over them."

It is possible that the thigh bone in which Dr. Plot was so much interested was not that of an elephant, for he says: "But what is *instar omnium* in this difficult point, there happily came to *Oxford* while I was writing of this, a living *Elephant* to be shewn publickly at the Act, *An. 1676*, with whose *Bones* and *Teeth*.¹ I compared ours: and found those of the *Elephant* not only a different Shape, but also incomparably bigger than *ours*, though the Beast were very young and not half grown."

The conclusion follows that the thigh bone is human

¹ The teeth were evidently those of a ruminant.

and belonged to a giant: many marvellous accounts of giants are then cited, some clearly fabulous, and not carrying conviction to the author himself, who proceeds as follows: "But to come closer to the Business, and more determinate Statures, the same *Pliny* tells us of two others living in the time of *Augustus*, nicknamed *Pusio* and *Secundilla*, whose Bodies were preserved for a Wonder in the *Sallustian* Gardens, that were ten Foot high; and that in his time, there was one *Gabbara*, brought out of *Arabia* in the days of Prince *Claudius* the Emperor, exactly of the height of *Goliath*, viz., nine Foot nine Inches high, which being a Size very proportionable to our Bone found at *Cornwell*, I am rather inclined to believe, that *Claudius* brought this *Gabbara* into *Britain* with him, who might possibly dye and lay his Bones here, than that ever they belonged to any *Elephant*: except we shall rather say that here also *Corinaeus*, Cousin to *Brute*, might kill one of *Gogmagog's* Race, and that from him the Place doth take its Name, as well as the County of *Cornwall*."

In reflecting on these views of Dr. Plot, no one will fail to recognise his perfect good faith; he does not appear to have been greatly influenced by theological prepossessions and is honestly anxious to arrive at the truth.

What then, it may be asked, was it that led him, and and many great naturalists with him, to conclusions so opposed to those of Steno, so opposed as we now know to the truth itself?

There were evidently several reasons. In the first place, Plot clearly perceived that the admission of the organic nature of fossils brought with it a whole train of perplexing consequences. To avoid these, he preferred an appeal to crystallisation, as raising ultimately fewer difficulties. Other branches of Science were not at that time sufficiently advanced to show the baselessness of this explanation, the subject was a difficult one, many additional observations were needed, and the mind of Plot was eminently critical. When we survey the vast superstructure that modern Science has raised on the foundation prepared by Steno, i.e. on the organic nature of fossils, and the super-

position of stratified rocks ; none but an impatient mind will feel other than grateful to Plot for having subjected these fundamental principles to the severest examination, for having spared no argument which could possibly be brought against them. Next to suggestive generalisation, Science stands in need of honest criticism. Geology required a prophet, and she found him in Steno, but she also required a critic, and in Plot she met with one of the most penetrating intellect and uncompromising spirit.

Both sides of this momentous controversy had now been fairly presented to the world, and it was left for the faithful labours of the next century to arrive at a decision. The principles of Steno eventually prevailed, and Geology entered upon a new phase of her existence ; she commenced to free herself from the trammels of time !

Hutton in his great work *The Theory of the Earth*, published in 1788, attacked the subject on that side where Steno had most conspicuously failed. His method was the same as Steno's ; just as Steno had explained fossil remains by comparison with living animals, so Hutton explained the past history of the earth by comparison with the present. If this county of Oxfordshire was, together with all England, at one time submerged fathoms deep beneath the sea, it was owing to a slow movement of subsidence such as Hutton considered was affecting islands and continents at the present day. Would we account for the marine sediments of which our land consists, we have but to turn our eyes to the rivers constantly bearing their burden of mud into the sea, where it is spread abroad in strata precisely similar to those of the remote past. As strata are formed now, so have they always been formed ; as lands rise and fall now, and seas pass imperceptibly into continents, and continents into seas, so they have always done in the past. But how slowly all these changes proceed ! A human lifetime is not long enough to witness any appreciable effect. In this period the thickness of stratified material laid down by a river in the sea would not amount to more than a few inches, and the rocks of the earth's crust are many thousands

of feet in thickness. Well might the stoutest hearted be appalled before the vast æons of time which Hutton conjured up before the imagination! Not so Hutton; having broken the narrow bonds of time previously imposed upon our science, he revelled in excess of freedom, and declared that the duration of our planet was practically infinite!

How was this new teaching received in Oxford?

The title of a work by Dr. Kidd, who was Professor of Chemistry in 1803, will inform us. It runs as follows: *A Geological Essay on the Imperfect Evidence in Support of a Theory of the Earth*, 1815; and the book itself concludes with the assertion that "ancient strata cannot be explained by existing causes: that the Science of Geology is so completely in its infancy as to render hopeless any attempt at a successful generalisation". In this opinion I believe Dr. Kidd to have been in sound agreement with the teaching of the leading geologists of the day: his work is by no means aggressive, and might rather be regarded as a text-book than a polemic: as a text-book it was well "up to date," and we are impressed on reading it with the great advance which had been made since the time of Plot.

Here must be mentioned the name of William Smith, a native of Oxfordshire, though not a member of this University. His various works were published between the years 1790 and 1815, and to him we owe a discovery that excited no controversy, but which nevertheless was the greatest in Geology made since the time of Steno. It was that different strata contain different kinds of fossils, which are peculiar to them, and thus serve as marks by which the strata can be identified; so that with the aid of fossils it has become possible to trace the same group of strata across the length of the British Isles, nay, even throughout the whole of Europe and into distant parts of the world. This discovery made Geology for the first time possible as an exact science.

The successor to Dr. Kidd was a geologist of the very highest rank, one eminently great amongst a crowd of great contemporaries. I allude to the famous Dr. Buckland.

The period of Buckland has been styled, and justly

styled, the Golden Age of Geology. Sedgwick was his contemporary in Cambridge, Phillips, afterwards to succeed him, was his contemporary in Dublin, Murchison learnt his first lesson in the field from him, Lyell was his pupil, Agassiz a coadjutor, and Conybeare his nearest friend.

When Buckland was appointed Reader in Geology, the foundations of the science were already laid, but great problems remained for solution. The question of the deluge and how far its effects could be recognised in the structure of the earth's crust was still one of these.

Buckland appeared at first as a champion of the deluge: thus in his inaugural lecture delivered before this University in 1819, he expressed himself in the following words: "The grand fact of an universal deluge at no very remote period is proved on grounds so decisive and incontrovertible, that had we never heard of such an event from Scripture or any other Authority, Geology of itself must have called in the assistance of some such catastrophe to explain the phenomena of diluvial action".

Subsequently pursuing his researches into this question he seems to have ransacked the whole world for evidence and found everywhere confirmatory proofs. The ancient gravels of Wytham were, he considered, swept from Warwickshire and counties still farther north, in the rush of the great flood; stones from Norway were carried to the east coast of England, and the mass of pebbles and other *débris*, which were driven along with it, scoured the face of the country and thus produced the polishing and striation so frequently visible on the surface of the harder rocks of our islands; the bones of mammoths and other mighty monsters of prediluvial times, that lie buried in caves and elsewhere, were eloquent in their testimony to the destruction which it wrought on the living world. That the loftiest mountains were submerged by it was proved by the discovery of the bones of horses and deer at an altitude of 16,000 feet above the sea on the slopes of the Himalayas. They are brought down by the avalanches of those mountains, and are said by the natives to have fallen from the clouds and to be the bones of genii. The *Reliquiæ Diluvianæ*, in which these

views appear, was published in 1823, and by its skill, learning, and eloquence at once attracted universal admiration.

But Buckland, though he appears in this work as an advocate, was by no means merely an advocate, his was a mind too highly endowed to rest satisfied with any but the most convincing proof, and as time elapsed, and he extended his researches, the evidence, which he so industriously accumulated, so far from strengthening his position, began to gradually undermine it, and already in 1837, when he published his great work on *Geology and Mineralogy Considered with Reference to Natural Theology*, we find him wavering. While still asserting the occurrence of a diluvial catastrophe, he was prepared to abandon the view, which would connect this with the Noachian deluge. "It has been justly argued," he writes, "that as the rise and fall of the waters of the Mosaic deluge are described to have been gradual and of short duration, they would have produced comparatively little change on the surface of the country they overflowed. . . . This important point, however, cannot be considered as completely settled till more detailed investigations of the newest members of the Pliocene and of the diluvial and alluvial formations shall have taken place."

Scarcely a year had elapsed after this opinion had been expressed when investigations were commenced, which were to cast a flood of light on the question from an unexpected quarter. Agassiz in 1838 was already engaged in those observations on the glaciers of Switzerland, and had commenced that series of brilliant discoveries which eventually culminated in a clear and reasonable explanation of the so-called diluvial phenomena. Buckland's study of these phenomena in Europe and the British Isles had rendered him the first authority of the time on this subject, though the cause of these phenomena, as we now know, was really not diluvial as he then imagined, but glacial. Buckland therefore was perfectly familiar with all the signs now recognised as characteristic of ice-action, no man more so: consequently when Agassiz took him over the glaciers of Switzerland, and showed him these agents actually at work,

smoothing, polishing and striating the surface over which they flowed, the evidence was presented to a mind already prepared to appreciate it, and a few days' personal investigation sufficed to convince Buckland of the truth of Agassiz' opinions. This was not a case of a mere tyro, introduced to the subject for the first time, but of a skilled and trained observer, familiar by experience with results, the cause of which he had for long been trying vainly to discover. Here however it may be better to allow Dr. Buckland to tell the story in his own words: among his MSS. I have found a rough draft of a letter, evidently written to Agassiz, in which he says: "In October, 1838, I for the first time attended to the effects of Glaciers, which you pointed out to me in the phenomena of polished and striated and furrowed Surfaces in the S. E. slope of the Jura, near Neuchatel, the origin of which, as well as the Transport of the Boulders of Granite, etc., from the Alps to the Jura, you referred to the Agency of ancient Glaciers. Not then satisfied with your Explanation, I proceeded to devote some days to the examination of actual Glaciers, and the result was my conversion from a sturdy Opponent to the adoption of your Theory as far as relates to Switzerland by the strict accordance which I found between many residuary Phenomena of existing Glaciers in the high Alps, and similar residuary Phenomena that are equally apparent on the S.E. slope of the Jura, fronting the Alps" [I mentioned to you] "that Sir J. Hall had in 1812 described similar polished surfaces, grooves and furrows in the vicinity of Edinburgh, and that other observers had occasionally noticed them on the surfaces of hard Rocks, which have been protected from the weather in many parts of Scotland and England. I also proposed to conduct you to some of the most remarkable of these spots on your visit to England in 1840, which you have just accomplished."

How justly Buckland estimated the importance of this great discovery is shown by notes in his handwriting, that were probably used to assist him in one of those remarkable speeches the echoes of which still reverberate in our time. In one he says: "For some time to come the Glacial

Theory must occupy a prominent place in Geological Investigation. The Subject appears to me the most important that has been put forth since the propounding of the Huttonian Theory: and the surface of the whole Globe must be examined afresh with the View of ascertaining how far the Effect of Glaciers and Ice-bergs and of Floods produced by melting ice and snow can be found and identified with the actual effects of Ice and Snow in our present Polar and Alpine Regions." This prediction has been verified; even now, fifty years after these words were uttered, observers are still engaged in the investigation of glacial phenomena; the subject still occupies as prominent a place as it did then, and promises to do so for an indefinite time yet to come. In another note Buckland comments on "The vast field of new Enquiry which the introduction of the Glacial period between our Epoch and the Newest Tertiary opens to the Geological Enquiry. The fact of the Greater Part of Europe and North America having for many years been sealed up under a cover of frozen Snow converted to the state of Glaciers is certain. . . . Thus the flood that caused the Diluvium which in my *Bridgewater Treatise* I have put back to the latest of the many Geological Deluges, was probably due to the melting of the Ice. The details of this Ice flood will fill a Volume and will constitute Vol. II. of my *Reliquiæ Diluvianæ and Glaciales*, which for fifteen years has been retarded for lack of the grand key which Agassiz has supplied in his *Etudes des Glaciers*."

Thus Buckland courageously recanted his earlier opinions on the deluge; and of the Noachian deluge as a geologic agent from this time forth we hear no more. This was one of the greatest benefits that Buckland conferred on Geology; commencing as the most powerful champion that had ever appeared on the side of the Noachian deluge, he step by step and by slow degrees was led to reconsider his position, till finally he abandoned it altogether, and left Geology freed from a mistaken doctrine to pursue her peaceful path, disturbed by no difficulties, beyond those naturally inherent in the sub-

ject. For this alone we owe him a debt of deep gratitude, but our obligations do not cease here, he introduced to the eyes of an astonished world a strange procession of extinct forms of life, with whom he seemed on most familiar terms ; parodising a line from Juvenal he exclaimed, “ Quicquid agunt ‘ Sauri,’ votum timor ira voluptas gaudia discursus nostri est farrago Libelli ” and then proceeded to describe “ the details of their political and domestic economy ”.

In his *Bridgewater Treatise* we may still read with interest and profit his vivid account of the Ichthyosaurs, Plesiosaurs, Megalosaurus, and Pterodactyles, and other monsters of the mighty past. This work was written sixty years ago, and yet such was the sagacity of the author in selecting only those facts which were well ascertained and sure, that it may be put into the hands of the youngest and most innocent of geologists, without fear of infecting his mind with forgotten error.

Nor must we here overlook his success in informing and convincing the world of science of the fact that in times immediately antecedent to the glacial period, our country and Europe generally was the home of a vast number of animals no longer found in it, some of them altogether extinct, such as the mammoth, rhinoceros, hippopotamus, cave hyæna, bears and tigers, and the arctic glutton.

Of Buckland's pupils, the greatest was Lyell, whose fame indeed overshadows his master's. Hutton had propounded the theory of the efficacy of modern causes, Playfair had illustrated it, and Lyell spent his life in defending and elaborating it. In consequence of his success in this work, it is far more often associated with Lyell's name, who was its foster parent, than with Hutton's who was its true originator.

We have already alluded to this doctrine. It is that the only method by which an explanation of the past can be obtained is by a careful study of the processes of the present, or, as it is often epigrammatically expressed, “ Geology finds in the processes of the present a key to unlock the past ”. It must be confessed at the outset that it is difficult to see where else she would find it ; not in the

future, of which we know nothing, and scarcely in the past itself, whose problems are to be explained. Epigrams are frequently only a neat way of obscuring a truth, in this case the fact would appear to be that most of the older observers, when they encountered interruptions in the succession of strata, were accustomed to explain them by interruptions in the orderly progress of nature, by sudden and violent changes, which were spoken of as catastrophes. Hutton and Lyell were able to offer explanations of these without the invocation of catastrophes. They certainly introduced the scientific method into cases which had previously been treated by a too free use of the imagination, and thus their doctrine, the doctrine of uniformity, has dominated geologic thought for the past sixty years, and will probably continue to do so for many years to come. The teachings of Hutton and Lyell have, however, another side; they assume not only that existing causes have acted in the past as in the present, but at the same uniform rate: this was the natural result of a reaction, which followed when Geology was loosened from her ancient bondage to time, and under the influence of which geologists came to regard the periods at their disposal as practically infinite. The mathematician often employs in his calculations the device of assuming some very large quantity to be infinite, and in this way obtains approximate results sufficiently close for working purposes. This is precisely what Hutton and Lyell did in their explanations. But when a limit becomes assignable to this quantity, the mathematician will abandon the fiction of infinitude and introduce the ascertained or estimated value into his equations with a view to arriving at greater exactitude. Of late years it has been asserted by a very high authority—Lord Kelvin—that a limit can be assigned to geological time; once more Geology is put under bondage, not however, as in her youth, tethered to a mere 6000 years, but free to roam through the ample magnitude of 30,000,000! It is at present impossible to say how near the truth Lord Kelvin's estimate may be; it is founded on data which may be inexact and on assumptions which may be illegitimate, but that it is approximately correct the preponderance of evidence seems to show.

That the strict uniformitarian view is as false in philosophy as it is unfounded on fact is an opinion which was shared by the late Professor Prestwich, and which is held by many of the Geologists of to-day, as it was by the great masters of old. It is tantamount to asserting that the progress of events on the earth can be represented by a curve which is a straight line. We should think strangely of the physicist who from the behaviour of a fluid through the range of ordinary atmospheric temperature should proceed to deduce from it a rate of expansion up to the temperature of the sun in one direction and down to absolute zero in the other; and yet this would scarcely show greater wisdom than the procedure of the geologist who from a knowledge of the earth's history during the past few thousand years should endeavour to deduce from it the rate of events during 30,000,000 of years in the past!

Dr. Buckland was succeeded by Professor Phillips, a man of most varied genius, a classical scholar, an expert mathematician, an omnivorous reader, facile both with pencil and pen, interested in all science and a master in his own. He taught in this University for more than twenty years, and during that period he enriched our science by numerous contributions of the highest value. A smooth and easy progress marked the course of Geology, and knowledge steadily enlarged its bounds. The great Cetiosaurus, one of the greatest of the old world monsters, larger even than the great Iguanodon which is now represented in our museum, we owe to him. Towards the end of his career, Geology like all other science was confronted by the re-appearance of an old and discredited doctrine, but now presented afresh with new and startling vigour, it was the doctrine of Evolution as expounded in the famous *Origin of Species by Natural Selection*. Once more an Oxford professor was called upon to pronounce judgment on one of those momentous questions which arise from time to time to disturb the steady current of established thought.

Darwin's present of a copy of his book was accompanied by the following letter :—

MY DEAR PHILLIPS,

I have directed Murray to send you a copy of my book on the *Origin of Species*, which as yet is only an abstract. I fear that you will be inclined to fulminate awful anathemas against it. I assure you that it is the result of far more labour than is apparent in its present highly condensed state. If you have time to read it, let me beg you to read it all straight through, as otherwise it will be unintelligible. Try not to condemn it utterly till you have finished it and reflected on the recapitulation. Not that I am so foolish as to expect to convert any one, who has long viewed the subject from an opposite point of view. I remember too well how many long years my own conversion took. The utmost which I hope is that you may see that more can be said on the side of mutability of specific forms than is at first sight apparent. If indeed your own observations have made you at all sceptical on the subject, then my Book may produce some effect. . . .

Yours very sincerely,

CHARLES DARWIN.

Phillips had for a long time previously given careful attention to the "Succession of Life on the Earth," and had chosen this subject for the Read lecture, which he delivered before the University of Cambridge, shortly before the appearance of the *Origin of Species*.

His pronouncement on Darwin's work was adverse. "Dead against," as Darwin wrote. His opinion as expressed in a letter to Darwin, of which he did not preserve a copy, called forth the following reply:—

ILKLEY WELLS HOUSE,
OTLEY, YORKSHIRE, 26th November [1859].

MY DEAR PHILLIPS,

Thank you for your note. Permit me to say one word about my book. Though many facts in Palæontology may appear, or be really, opposed to my notions, and though my explanations may be quite fallacious, I earnestly beg you to consider whether a theory wholly false would explain, as it seems to me to explain, several classes of facts—as affinity of inhabitants of islands to nearest continent; the nature of the inhabitants of oceanic islands; the affinities and classification of organic beings and their arrangement in groups; the strange fact of a member of one group being adapted to the habits of another group; the facts of morphology or homology; embryology and rudimentary organs. If you think the theory of Natural Selection does not to a large extent explain these classes of facts, I have not a word to say. Pray forgive me saying a word in favour of my own offspring to one whom I consider an important judge.

Yours very sincerely,

C. DARWIN.

That Phillips betrayed no bigoted opposition to the doctrine of Evolution is shown by several attempts which he himself subsequently made to construct a phylogeny of different groups of animals from a knowledge of their fossil remains, but while he succeeded in tracing several interesting lines of descent among species, he confessed himself unable to bring the more widely separated groups or genera into an ancestral connection. Since these early attempts of Phillips, we have learned not only to affiliate species and genera, but even families and orders, and the frequent discovery of missing links offers the most striking testimony to the truth of the theory of Evolution.

That Phillips was thoroughly justified in his position towards Evolution is suggested by the fact that even Huxley, the most philosophic advocate of the theory, fully admitted that at the time of publication of the *Origin*, Palæontology lent to its doctrine no support.

An argument which evidently had great weight with Phillips, in his rejection of the theory of Natural Selection, was the excessive duration that it postulated for geological time. This still remains an argument of weight, so that some biologists impressed with the vast periods which the Darwinian theory demands for its operations, are prepared to measure geologic time by Darwinian requirements. On this subject Huxley expressed himself with his usual wisdom, perceiving plainly enough that biologists are in possession of no data as to the rate at which species may become modified—were we to judge the past from the present we might have to admit that they do not become modified at all—he referred biologists to the geologist, telling them in pithy words that they must take their time from the geological clock.

Phillip's position towards the Darwinian theory seems to me to have been altogether a wise one; since his time the doctrine of Evolution has obtained universal acceptance, but Darwin's theory is still a battle-ground for contending opinions.

And though we are compelled to call Evolution to our aid when we attempt to explain the facts of palæontology,

yet it is well we should bear in mind that Evolution does not advance us far. It is a great generalisation, as was the maxim that "Nature abhors a vacuum," but like this maxim it stands itself in need of an explanation.

Suppose by some ingenious cinematograph we could recall in a continuous picture the succession of vanished forms of life, and watch the transmutation of animals as they passed from formless protoplasm to man; the spectacle would be interesting, but would the fact it displayed be one whit less a miracle than that we witness every time we plant the seed in the ground and watch the green shoot spring forth, to produce in due time the secret buds which unfold in leaf and flower?

Or even suppose we take the one step farther and admit the Darwinian explanation, with its three factors of selection, variation and inheritance, are we much nearer to the ultimate truth? A certain Paley once considered a certain watch; the watch has grown so old-fashioned since Evolution attained its vogue that it is now rarely seen in polite society, but it did great service in its time; permit me on this occasion to draw attention to another analogy,—to the curious similarity that exists between the progress of human invention and the evolution of organic species. From the great kingdom of locomotive machines a lowly stirp arose known as the "hobby horse"; it lingered on unregarded for a while, and then was superseded by a new species technically called the "bone-shaker". This gained a place in the world, multiplied rapidly, and became represented by a great number of individuals; as it multiplied it gave rise by a process called "variation" to a number of related forms, such as the "ordinary bicycle," the "extraordinary," and the "kangaroo"; many of these species of machine exhibited a decided advance in organisation upon their ancestor; a process of selection then set in, the fittest types survived, the unfit became extinct; the good qualities, which had secured this success in life, were handed down by a process of inheritance; variation of these successful types produced others still more successful, and selection, always operative, continually weeded out the less fit, till

one sole survivor now remains, the summit of evolution is reached in the genus "bicycle," species "safety," variety "pneumatic"! Just such a kind of succession may be traced in the series of animal remains preserved in stratified deposits, a few simple forms appear, drag on a lowly existence for a while, and then unexpectedly blossom out into an infinitude of varieties; with lapse of time these become thinned out, but organisation all the time advancing there results at last some happily endowed form, which supplants the inferior competitors, and becomes lord and master of creation.

In the case of the machine we think we know the causes of variation and inheritance, we attribute them to the action of the human mind. In the case of the organic world, we assert that we know not to what these processes are due; and yet, somehow, to my thinking there stands behind them, connected in some incomprehensible manner, but still connected, the working of a mind which is Divine.

Professor Phillips was succeeded by Professor Prestwich, the Nestor of English Geologists, who maintained the principle of Evolution in Geology, as opposed to Uniformitarianism; and to him followed Professor Green, whose loss is still fresh and still deplored.

The influence of Oxford on Geology has been far greater than in this imperfect sketch can be made to appear: she has never shut her eyes to a new theory but, whether by criticism or advocacy, has endeavoured to establish the truth that it might prevail; her direct contributions to knowledge have been very many and very great; if we mentioned no other work but one; that of *The Principles of Geology* by the immortal Lyell, would alone suffice to vindicate her fame.

W. J. SOLLAS.

ON PROGRESS IN THE STUDY OF VARIATION.¹

(*Continued.*)

WE have now to examine the special case of local varieties or races, and to consider how the phenomena they present may best be turned to account in the attempt to investigate objectively the origin of species. It is here that we are especially dependent on the efforts of the collectors. Without great collections little progress can be made, but by modification of the usual practice their utility for the solution of these problems might be much increased.

It is a fact familiar to every naturalist that in very many species individuals living in different areas are dissimilar, and that by these dissimilarities the species may be broken up into local races. This phenomenon of local differentiation is so common that species in which it is not in some degree apparent may almost be regarded as exceptional. The differences may be exceedingly slight, and appreciable only to a person who has had long experience of the form in question ; or, on the other hand, they may be so decided that it is only after special study and examination of series of specimens from many localities that the local races can be recognised as belonging to the same species.

In addition, too, to cases of this latter order, in which reason has been found for uniting dissimilar local races, there are numberless instances in nearly all orders of animals and plants where it is practically certain that forms which, on account of their dissimilarity and distribution have been considered as distinct, might with almost equal propriety be regarded as local races of the same species. Local differentiations of this kind are the despair of the systematist. How may species be distinguished from local forms ? How great must the differences be, and in what organs must they ap-

¹ Corrigenda to part i. of this article, which appeared in the last volume of SCIENCE PROGRESS.

P. 556, line 3 ; *for* "most subalpine" *read* "most southern".

P. 556, line 7 from bottom ; *delete* "wings".

P. 565, line 4 ; *for* "*monacha*" *read* "*eremita*".

pear, to justify the constitution of a species? On these weary questions volumes of controversy have been spent. No general rule can be found, for no real distinction exists, and in practice each man must follow his own standard. So long, however, as each distinct form receives a separate name by which it can be known and treated of it matters perhaps little whether it is reckoned as a species, subspecies, "representative species," local race or variety. All that we have to remember is that these various terms have no precise signification.

The existence of such local forms is nevertheless one of the best possible points of departure for an attempt to study the origin of species. In each case, where two local varieties are known, the problem of the origin of species is presented in a *particular* form. How did those two particular varieties A and B come into existence?

Causes apart, by what steps in descent was the one produced from the other, or both from something else? These are nascent species if there are any on the earth. Here if anywhere is our chance to see, if not the mode by which species come into existence, at least the way in which differentiated forms are connected together, and the steps by which they may separate. It is true enough that even in the case of most local races which are materially distinct the evidence as to the connecting steps is gone. In one locality one form is found, in another locality another form. Each area is distinct and isolated, and has its distinct population; beyond that nothing is known. But besides these there are other cases, rarer perhaps, but still fairly numerous, in which local forms inhabit conterminous areas, and, though distinct enough in their chief habitats, yet meet each other and breed together in the intervening district, producing offspring which we have no reason to suppose infertile. Such cases have a prerogative claim on the notice of the evolutionist. Two forms, each well defined, each, as is presumed, adapted by its peculiarities to its own area, meet in an intervening area. In what state is that intervening population? On seriation does it appear that the population consists mainly of one normal

form, the mean between the two others, or is the intervening population found to be practically divisible into two groups of individuals, the one more like the one race, and the other group more like the other race? In any such case there is in fact an opportunity of seeing "swamping" by intercrossing, of getting evidence as to whether the two races are capable of freely blending, or whether there is any discontinuity between them.

In regard to some of the more familiar examples, certain of which are spoken of below, discussion has arisen on the question whether the population occupying the intermediate area where the two races intergrade should be regarded as hybrids between the two, or rather as a still undifferentiated population. A third possibility is that the one race is being or has been directly formed from the other by discontinuous variation, and that in the area of intergradation the process is going on. On consideration, however, it will be seen that, whichever be the true account, the discussion must be a barren one; for if there is discontinuity between the two forms, whether the two races are newly met or newly dividing the appearances would at any one point of time be the same. This question, like so many others concerning evolution, could only be answered by appeal to observations made at several moments separated by intervals of time. Such evidence is, of course, wanting. Nevertheless, for our purpose a knowledge of the truth of one or other of these views is not immediately needed. The essential fact is that the two forms, though distinct enough to pass for separate species, did they not occupy the same area, on overlapping interbreed in nature. Though in the ordinary sense of the term these forms are not "species," yet they have many of the attributes of species.

If it is true that the evolution of the one form from the other, or of both from something else, has proceeded by a long series of insensible steps, of which each in turn has been a normal in its own day, surely we should expect to find the intermediate area occupied by an intermediate population, having an intermediate (if not the mean) for its nor-

mal. More especially should we expect to find this state of things in those instances in which the two local races are what are called climatic varieties, varieties that is to say associated with conditions which we can recognise as distinct (without postulating any direct relation of cause and effect). It is not rare, for instance, to find a species represented in the North by a northern race, and in the South by a southern race. In such a case, if the two forms inhabit a continental area, divided by no natural barrier, we should expect, on the hypothesis indicated, that in the intermediate area there should be an intermediate normal. Or, more precisely, we should expect in travelling from North to South to pass through country inhabited by a whole series of normals, passing in unbroken continuity from the distinctly northern form to the distinctly southern.

Again, if a lowland species is represented in the mountains by an alpine race we should expect as we travel up the valleys to pass through a series of normals, each appropriate to its own level. We may concede that a full chain of intermediates as normals need not be expected in every case, but that the state of things ought, on the whole, to bear out this expectation is surely clear, and if the hypothesis of an essentially gradual evolution is true, the geographical transition from the one race to the other should be essentially a gradual transition.

It is very likely that cases could be produced where such essentially gradual transitions occur, but, as will be shown, there are others which cannot readily be so described. Whether there are any general features distinguishing the two classes of cases we cannot yet say. It is the object of these remarks to call attention to the paramount importance of such phenomena as subjects for investigation.

Few indeed are the instances which we can yet with confidence refer to either class. Collectors and systematists have hitherto been content as a rule with the bare knowledge that there is sometimes intergradation between local races. Statistical evidence of the modes of intergradation are almost entirely wanting. Commonly the information is of the most meagre description ; and even in the case of

easily accessible forms no collections or experiments have yet been made with a view to answering a question of such peculiar interest to the evolutionist.

In the following pages I propose to take a few illustrations—most of them already well-known to specialists—showing the kind of phenomena which are thus open to observation. The methods that should be applied are essentially the same in each case, consisting in :—

- (1) The collection of large samples taken at random in the area believed to be occupied by the pure races.
- (2) Separate seriation of the individuals of each sample according to the degree in which the differentiating character is presented.
- (3) Similar collection and seriation of similar samples from a series of stations connecting the areas occupied by the pure races.
- (4) In suitable cases experimental crosses between the pure forms and between each pure form and such intermediates as can be obtained.

It is unnecessary to say that in planning observations on these lines, special regard must be paid to the possibilities of error due to migrations and other sources of error affecting particular cases.

Local races may be distinguished by differentiation in respect of various bodily features, though naturally the most familiar are those which are distinguished by some conspicuous and easily recognised characteristic, such as peculiar stature or proportions, colour, sculpture, and so forth. But there are several well-known instances where intergradation occurs between races distinguished from each other by what are called “anatomical” characters. A good instance of such a phenomenon is that of a species of *Cistudo* in N. America, which is represented in New England by a form with four toes on the hind foot, while Mexican representatives have three. This form was originally considered by Gray as a separate genus, afterwards as a separate species. Subsequent evidence has shown that the fourth toe fades away so gradually that the

two forms are clearly one species. Specimens have also been seen with three toes on one foot and four on the other.¹ These animals are so common that a statistical study of their variation in nature should not be very difficult to make.

Among birds some of the most striking examples are known, and have been the subject of a good deal of discussion which, for the most part, has followed exclusively the lines indicated above. One of the best known is that of the species of *Colaptes* (a Woodpecker) inhabiting the United States and Mexico. In this instance the facts have been examined in considerable detail, especially by J. A. Allen, (1) who has given a careful summary of the evidence derived from the study of a large mass of material. As appears from his observations the case is briefly this. Omitting forms with more restricted range, two species of *Colaptes*, *C. auratus* and *C. cafer*, occupy nearly the whole of N. America. A band of country 300 to 400 miles wide extending from British Columbia to Texas is common to the two forms. North and East of this belt *C. auratus* is found unmixed, while *C. cafer* in the pure form occupies most of the country south and west of the common area. The distinctive characters are very striking, the most noticeable being the following :—

<i>C. auratus.</i>	<i>C. cafer.</i>
1. Quills yellow	1. Quills red.
2. Male with black malar stripe.	2. Male with scarlet malar stripe.
3. A scarlet nuchal crescent in both sexes.	3. No nuchal crescent in either sex.

Besides these there are several less conspicuous differences, one of the most singular being due to the fact that the colours of the crown and throat are *transposed* in the two forms, *C. auratus* having a grey crown and a brown throat, while *C. cafer* has a brown crown and a grey throat.

In describing the “ever-varying combinations” of these characters found in the area where the races overlap Allen states that the specimens range from individuals which show

¹ I have referred to the evidence in detail, *Materials*, No. 609.

the characters of one race with only a trace of the other to birds in which the two sets of characters are about equally blended :—

“ Thus we may have *C. auratus* with merely a few red feathers in the black malar stripe,¹ or with the quills merely slightly flushed with orange, or *C. cafer* with either merely a few black feathers in the red malar stripe, or a few red feathers at the sides of the nape, or an incipient, barely traceable scarlet nuchal crescent. . . . The quills may be orange yellow or orange red, or of any shade between yellow and red, with the other features of the two birds about equally blended. But such examples are exceptional, an unsymmetrical blending being the rule, the two sides of the same bird being often unlike. The quills of the tail, for example, may be part red and part yellow, the number of yellow or red feathers varying in different individuals, and very often in the opposite sides of the tail in the same bird. The same irregularity occurs also, but apparently less frequently, in the quills of the wings. . . . A bird may have the general coloration of true *cafer* combined with a well-developed nuchal crescent, or nearly pure *auratus* with the red malar stripes of *cafer*. . . . Or we may have the general plumage as in *cafer* with the throat and crown as in *auratus*, and the malar stripe either red or black, or mixed red and black, and so on in almost endless variations, it being rare to find, even in birds from the same nest, two individuals alike in all their features of coloration.”

Now, though from this account it appears that the several characters may combine in varying ways, the great irregularity and especially the asymmetry of the combinations are strong indications that there is not free blending of the characters, but rather that though they may co-exist in the same individual they are in some degree alternative, forming, in fact, a kind of patchwork. Though even in this—one of the best known cases—adequate statistics are still wanting, it seems to be clear that there is no great population with either the mean or any other intermediate form as a definite normal.

A more complex example is provided by species of the genus *Quiscalus*, the Boat Tail Grackle of N. America. The facts are indeed so complicated that they cannot be represented in a brief statement. A full account of this matter is given by F. M. Chapman (2) who has made as far as

¹ A Californian specimen in the Cambridge Museum shows these feathers partly black and partly red. Each feather though black in its basal parts has a red tip, the black pigment being apparently transmuted into red pigment.

his material permitted, a statistical tabulation of results. Here again the variation is in colour of plumage, which may take various dark metallic shades not easily described in words. Concomitant variation in size and proportions are also shown to occur. The facts as stated by Chapman are, broadly speaking, that *Quiscalus æneus* has a breeding area from the Rio Grande Valley northwards to British America, and north-eastwards to New Brunswick. In the area south-east of this *Q. quiscula* occurs, ranging from Florida to Massachusetts. In Pennsylvania and Massachusetts (probably also in a belt of country extending south-west from Massachusetts through Pennsylvania to the north of the Mississippi) the two forms meet and intergrade. In the rest of its range *Q. æneus* is remarkably constant. The complexity arises from the fact that *Q. quiscula* occurs in a variety of forms which can be regarded as three phases of coloration. Phase No. 1 is found in S. Florida associated with Phase No. 2, and many intermediate forms. At the north of its distribution Phase No. 1 is absent, No. 3 being there chiefly found, associated with No. 2 and all intermediates. In the central part of its range all three phases and intermediates occur together. The transition from *æneus* to *quiscula* occurs exclusively through phase No. 3 of the latter.

From the particulars given it seems likely that though the transition is gradual in the sense that all intermediates occur, being indeed frequent in the area of intergradation, yet here again the connecting links are not a series of normals following each other in succession along lines passing from the area of one race to that of the other. Another example well known to ornithologists is that of the Rollers, *Coracias indicus* and *affinis* (see Dresser (3) and Sharpe (5)). Here again the coloration of the two races or species is quite distinct. *C. indicus* inhabits the Indian Peninsular generally, extending westwards to Asia Minor, while *C. affinis* belongs to British Burmah and Indo-China. It spreads, however, westwards to the neighbourhood of Calcutta and onward to Sikkim and Nepal. This part of its distribution is common to the two forms, and intergrading

occurs. A considerable series of intermediates from this region is preserved in the British Museum, and from these and others described by Dresser it appears that here again there is no definite race of intermediates grouped round the mean as a normal, but that the intermediates are a heterogeneous body, and highly variable. It is quite possible that on seriation they would be found to be on the whole divisible into two groups favouring the two types respectively.

Birds, however, are not well adapted to statistical treatment of the kind contemplated. Their large size, frequently migratory habits, and the comparative difficulty in preserving them in large numbers makes them for the most part unpromising material for such work. Besides this, it is in very exceptional cases only that they can be bred in captivity. Before passing to other examples, however, it may be noted that in these cases, though the individual parentage of the intermediates or of the types in the area of intergradation is scarcely as yet a matter of observation, there is nothing in the facts inconsistent with the possibility that the whole phenomenon may be one of discontinuous variation. Nevertheless, it is quite as possible that the intermediates may in each case be the result of cross-breeding. If the latter is the true view, then, since the whole population is not found to have regressed to the mean form, this state of things must be due either to the existence of some principle by which the types mate more often legitimately than illegitimately, or to the existence of natural discontinuity, or to the unfitness and consequent extermination of the mean forms. The last hypothesis is so improbable that it need scarcely be considered. No support could be produced in behalf of such a view, either from facts of the case or analogy of other cases.

Among Lepidoptera there is perhaps more hope of obtaining direct evidence on this question, and there are numerous cases which would well repay careful work. Scarcely any have been touched as yet.

A remarkable example has been observed by W. H. Edwards (4) among the N. American Papilios. The form

known as *P. oregonia* is a species much resembling *machaon* in general coloration. It is found in Washington Territory and British Columbia. Another "species," *P. bairdii*, has the upper surface largely covered with black scales, especially in the female, and in general appearance may be said to resemble the *asterias* group more than the *machaon* group. It occurs in Arizona without intermixture with *oregonia*, just as the latter occurs in the north without intermixture with *bairdii*. However at Glenwood Springs in Colorado *oregonia* and *bairdii* were found flying together by Edwards.

By rearing eggs laid by the Glenwood females, he found that both forms could be reared from females of either form, eggs from *bairdii* females producing both *bairdii* and *oregonia*, and eggs from *oregonia* producing both *oregonia* and *bairdii* (statistics given). The experiments showed that this is not merely a case of seasonal change, both forms occurring in each brood.

For our present purpose it is important to notice the fact to which Edwards calls attention that the *oregonia* and *bairdii* in the locality of intergradation were not for the most part the pure forms as they occur in areas where there is no intermixture. Some of the *bairdii* were typical, but most departed in different degrees from the type, no two being quite alike. The *oregonia* also departed from the type in the direction of *bairdii*. Nevertheless as the whole account shows, here there were two well-marked forms like those elsewhere known as local species or races, breeding together in one locality but not regressing to the mean form.

I will now refer briefly to two examples of marked local races, whose relations I have been myself endeavouring to investigate. The cases are those of *Pieris napi* (the Green-veined White Butterfly) and its Alpine var. *bryoniæ*, and *Pararge egeria* (the Speckled Wood Butterfly) and its northern form *egeriades*. I have been collecting and crossing these species for the past three seasons, and am still far from arriving at definite results, but I mention the subject here in the hope that others may be induced to look out for some of the points to be observed.

As regards these cases the history is far too complex to be treated as a whole in the present paper. Difficulty arises not only from the great extent of country inhabited by the species and their varieties, but also in the case of *napi* from seasonal dimorphism. *P. napi* in this country is too well known to need description. Here as a rule the summer brood do not differ greatly from the spring brood, though exceptionally they may do so. The summer form when it is distinct is called *napææ*. It is chiefly characterised by the large size of the two spots on the fore wing in the female, and by the great reduction in the amount both of dark venation on the upper surface of the fore wing, and of green venation on the lower surface of the hind wing. Such a distinct summer form abounds in the lower parts of the valleys in the Alps and of course in most of the warmer regions of Europe.

At and above 3000 feet or rather less, *bryoniæ* may often be met with in those valleys which descend from the high mountains. It is in the *female* that this variety is clearly marked, being characterised by the presence of bands of brown scales following the nervures or the upper side of both wings, especially in the fore-wing. The male closely resembles the spring male of *napi*, and is not generally, if ever, distinguishable from it with certainty. *Bryoniæ* extends up to 6000 feet. Whether this alpine form ever occupies any considerable area without *any* admixture of *napi* I do not know. As regards particular hillsides I think it does, but my experience is that colonies of *napi* may occur up to nearly 6000 feet, especially near chalets, appearing in summer broods identical with those of the lowlands. That in such places the two forms occupy more or less distinct spots, is, I think, probable.¹ Lower down, however, *napi* and *bryoniæ* may in many places be taken flying freely together, but in such localities, according to my experience, intermediates are of exceptional occur-

¹ It is possible that the two forms in such cases live on different plants. The chief food of *bryoniæ* at Tosa is *Biscutella lævigata*, but both forms eat many kinds of Cruciferæ, and I cannot find that the colours are changed by difference in food.

rence and are less common than the type forms. Why is this?

The case is an interesting one in several ways. Weismann has made this species the subject of a good deal of speculation and some experiment. In his earlier paper (6) he regards it as certain that *bryoniæ* is an ancient form dating from the glacial period and that *napi* has been very gradually evolved from it as temperate conditions supervened. In the later paper (7) from the fact that two male *napi* and a *bryoniæ* female irregularly patched with white emerged from some forced pupæ supposed to be all offspring of *bryoniæ*, he is apparently disposed to doubt his original view, though, as he points out, the *napi* may have been merely introduced larvæ and the blotched specimen may have been gynandromorphous. The possibility that there may be discontinuity is not considered. Moreover, the matter is greatly complicated by the fact that *bryoniæ* is not, as Weismann states, exclusively one brooded, for I have reared summer broods, emerging in August, both from *bryoniæ* females caught at Fobello (3000 feet) and above the Tosa Falls (5500 feet). The former were flying with *napi*, and for anything I know these summer emergences may have had a *napi* father.

Those from Tosa may also have been cross-breds, though this is less likely. All the second brood from Fobello were intermediate and some of those from Tosa are so also, though the rest of the latter are *bryoniæ*. It appears therefore to be possible that the intermediates found wild may be a second brood from *bryoniæ*. In order to determine this it would be necessary at least to have statistics from one locality extending over the whole period during which the species fly.

The view that *napi* has been very slowly evolved from *bryoniæ* or *bryoniæ* from *napi* is not easy to reconcile with the facts of the present occurrence of the forms. *Bryoniæ* is met with in the far North and again in the high Alps. If *napi* has been continuously evolved from it, passing through a long series of intermediates as normals, we should expect that those places in Europe which have a climate inter-

mediate between glacial conditions and temperate conditions would have a normal population of intermediates. On such points the systematic treatises are not very reliable; but I cannot discover that such a population exists anywhere. With regard to Norwegian and Arctic specimens the accounts are somewhat contradictory, and the individuals I have seen are too few to warrant a definite opinion. The same applies to Central Asian forms. Certain it is that in the Alps on passing from the lowlands to alpine conditions a population having an intermediate as its normal form is not met with in June and July. At this time in such localities the common forms are *napi* of the second brood, *bryoniæ*, and occasional intermediates which may be either pure-bred second brood *bryoniæ* or cross-breds, or both.

It is to be hoped that some who have the requisite opportunities will make collections of large samples of these forms, either in the North of Europe or in alpine regions, and so contribute to the solution of this very attractive problem. It will be understood that the account here given is the merest outline of the facts.

It is interesting to reflect that if we had known nothing of the history of the origin of *A. betularia*, var. *doubledayaria*,¹ and if we had not actual historical evidence that it has replaced the type in the North of England we should most certainly have been told that the one had been very gradually evolved from the other in the course of ages. The same example shows moreover that there need be no natural distinction between the case of a variety discontinuously occurring with the type and the case of two local

¹ To the experimental evidence of discontinuity in this case I should have added the following reference, E.R. Bankes, *Ent. Rec.*, vii., p. 181. Since the first part of this article was published, Mr. W. H. B. Fletcher has kindly lent me the entire brood produced from a black female, reared in captivity, which was tied out at Worthing where only the type is known. There can thus be no doubt she was fertilised by a *betularia* male. The offspring are sharply divided as follows: *betularia*, ten males, eight females; *doubledayaria*, six males, five females. Curiously enough one of the *betularia* is an abnormally light specimen. The rest are either normal, or else completely black all over.

racés occupying distinct areas. We might suppose that the former case is in reality comparable with familiar examples where there is an apparently constant dimorphism of one sex, or with the reciprocal dimorphism of many flowers, in which it is believed that the dimorphism is a permanent attribute of the species as much as the differentiation of parts in the body of an individual. The case of *betularia* shows how the one may pass into the other.

Before leaving the subject I will give one more example, to which I have been paying some attention, because it is especially one where co-operation is needed. I take this opportunity of thanking M. René Oberthür for kindly sending me specimens, and for other assistance rendered me in regard to this species. The Speckled Wood butterfly of England is now well known to be a geographical variant on the form inhabiting France and Spain generally, the Mediterranean littoral and a great part of the rest of Europe. In the South the pale yellow of our own insect is replaced by a bright fulvous yellow. The southern form is now called by the strict rules of nomenclature *Pararge egeria*, while the British form is the var. *egeriades*. Omitting other complications, our problem is raised in the simplest form by the facts of the distribution of the varieties in the French and Spanish continent. At Gibraltar *egeria* flies alone. Practically similar *egeria* flies alone in the Basses Pyrenées (as also at Avignon and Tarascon) and in all the country northwards as far as Poitiers. About sixty miles north of this point, at Tours in the Loire Valley, country not noticeably different, there is a very different form which is almost exactly intermediate between the two chief varieties. This intermediate form is spread over Brittany and Normandy. Those from the neighbourhood of Paris are of the same intermediate form, or perhaps a little paler. As far as my own observation goes, there are essentially *three* forms, southern, northern and intermediate. From between Poitiers and Tours I have no specimens, and it would be interesting to know whether in that region the southern form shades off into the intermediate. Similarly it would be interesting to have col-

lections from places where the intermediates might be expected to shade off into the northern form.

It is certainly likely that there may here prove to be a true case of continuity, but so far this is not quite clear, either from the wild specimens or from those that have been reared by crossing. The cross between the northern and southern types usually produces the intermediate, agreeing exactly with Breton specimens, but on recrossing these with the southern form my evidence inclines, but by no means certainly, towards the conclusion that there is not complete blending between the two. I anticipate, on the whole, that fuller investigation will show that there is complete or almost complete continuity. The abrupt change on passing from Poitiers to Tours is, however, scarcely consistent with this anticipation.

The insect is very local, and probably wanders hardly at all. It is therefore a good subject for this inquiry. The west side of France is most suitable ground for the investigation, for here there are no mountain ranges or other barriers.

In mountainous parts of the country the true *egeria* is always replaced by paler forms. Such forms are found, for example, at Chambéry, and these are not strikingly different from those of Brittany, though on the whole inclining to the fulvous type, while from Doubs M. Oberthür has sent me specimens precisely like English ones.

Cases which might repay statistical study in this country are, amongst others, those of the varieties of *Polyommatus agestis* and the two forms of *Fidonia piniaria*. The latter is very probably a case of discontinuity. In examining it difficulty may arise, as Mr. Fletcher has pointed out to me, from the possibility that with the recent plantation of the Scotch Fir in many parts of the country artificial introductions may have taken place. I should be grateful for the loan of series of specimens or for accurate records of the occurrence of these forms.

The illustrations here given will suffice to show lines on which the objective study of the origin of species may, as it seems to me, be profitably pursued. Until work of this

kind has been carried out in many cases, both statistically in the field and by cross-breeding in captivity, we have not got the material even for a preliminary survey of the relations between species and variety. Such work is pre-eminently to be commended to collectors and systematists. It is for want of it that so little progress has been made with these questions. It is depressing to see how those who are engaged in the business of systematic work often neglect to give the essential particulars as to the variability of the material submitted to them for description. That such a character is "variable," or "so variable that no reliance can be placed on it" is often all that we are told, when in many cases with little additional trouble the number of specimens exhibiting each variation could have been recorded, thus greatly lightening the task of those who come after. If collectors and systematists would arrange their work in such a way as to bring out and not conceal the objective phenomena of evolution, and if the evolutionist would appreciate that the proper way to study the relation of type and variety is to take up the work at the place where the systematist leaves it, we should have that partnership between the two classes of naturalists for want of which so much effort is wasted and progress is so slow.

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W. BATESON.

PREHISTORIC MAN IN THE EASTERN MEDITERRANEAN.

PART II.

THE MYKENÆAN QUESTION.

45. **A** RECENT article summarised the evidence of the course of the dominant civilisation in the Eastern Mediterranean down to the point where Hellenic history may fairly be said to begin. But the caution is perhaps hardly yet superfluous, that community of civilisation does not prove community of race, though it frequently accompanies it, and sometimes can be shown to result from it. The question therefore which next occurs is twofold : firstly, from what source or blended sources is the Ægean civilisation derived ; secondly, if it is indigenous, what is the ethnographic position of the race or races who originated it ; and if it is immigrant, who, in addition, introduced it, and from whence, among the peoples of the Ægean area and their neighbours.

46. A premature but not unnatural conjecture led Dr. Schliemann to equate absolutely the civilisations and peoples of Mykenæ and what we know now as pre-Mykenæan Hissarlik with those described as Achæan and Trojan respectively in the Homeric poems ; and a large part of the subsequent commentary on these and kindred discoveries has approached the question from the side of the Homeric literature, and has turned upon the validity of this identification. But this identification itself only substituted an Achæan y for the Mykenæan x , and the problem still remained of the relations in which Homer's Achæans stood to their namesakes in historic Hellas, and to the other Hellenic stocks.

47. Dr. Tsountas has attempted to dissever ethnologically the representatives of the Cycladic from those of the Mykenæan stage of Ægean civilisation, and to identify the former with the Danaans, the latter with the Achæans

of the Homeric poems, whom he regards as incomers from the north, and responsible in particular for the replacement of the Cycladic and early Mykenæan rite of burial in cists or shaft graves by the later Mykenæan usage of chamber burial.

48. The same conception of the Achæans as a dominant caste of immigrant northerners, superimposed on an aboriginal population who had the Ægean culture already in a high stage of development, has been amplified in special relation to the literary question by Dr. Leaf; and, as explaining the gradual acceptance of the Achæan tribal name Hellene as a national designation, by Professor Bury; while in identifying the pre-Achæan aborigines with the Pelasgians of Hellenic tradition Professor Ridgeway has perhaps succeeded in substituting, yet again, a Pelasgian z for the Achæan y ; but perhaps also in the process has contributed in detail to the eventual determination of both unknowns.

49. But behind the Homeric Question, which is really the question of the relative date of the Homeric Poems themselves or their essential constituents, stands the main question at issue, how far the Mykenæan civilisation should be regarded as indigenous at Mykenæ and on other Greek sites; how far as having been imported more or less ready made. For the great differences between the Mykenæan and the Hellenic in almost every department of art and culture made a transition in direct series from one to the other at first sight almost inconceivable, especially in view of the admitted break both in the ancient Hellenic tradition, and, until lately, in every class of evidence for the intermediate period.

50. The fact that Dr. Schliemann happened at the outset, at Hissarlik and Mykenæ, upon two of the materially wealthiest Ægean sites hitherto known, and in the latter case upon one which until 1896 remained wholly unchallenged for the variety and magnificence of its remains, made the indigenous growth of a civilisation, with a character so pronounced and resources so ample, even more difficult to reconcile with preconceived ideas than it might have been if minor and sporadic discoveries had prepared the way

for the recognition of these masterpieces. And the new material appeared at a moment when literary and philological criticisms had in many quarters combined to weaken the authority of the Hellenic, and particularly the Homeric tradition of a Golden Age, of which Hellenism should be regarded as a Renaissance. Consequently the ingenuity of archæologists has been largely directed rather to the indentification of foreign and imported elements, in the later stages of Ægean culture, than to the affiliation of these to an indigenous parentage, and to the conception of Ægean culture as simply that corner of a larger Mediterranean and European culture which was most favourably placed for expansion on its own lines, but not to be regarded as distinct from that larger whole, simply because in its later stages it had so expanded.

51. The large crop of theories of Ægean origins may be roughly divided into three groups. If Ægean civilisation was not mainly indigenous—and this was for a while the prevailing impression—it must either have been derived from an Asiatic, or an African source. If from an Asiatic, two routes seemed to be open, by the land route of Asia Minor and the eastern coast of the Ægean, or by the Syrian and Phœnician coast; these being the two main lines of communication with the Euphrates Valley which remained open and in use at the beginning of Hellenic history. If on the other hand from an African, which before the detection of an early Libyan civilisation meant an Egyptian source, then the route must either have been again *viâ* Phœnicia, or more directly along the north coast of Africa; but in spite of Homeric indications, the latter does not seem to have been seriously considered until quite recently. To take the Anatolian theories first and then the Syro-Phœnician :

52. The “Karian Theory” of Drs. Köhler and Dümmler rested upon Hellenic traditions of an occupation of many of the islands, some part of Crete, and several points of the mainland of Greece, by Karians, or according to another version by Leleges from Karia. This thalassocracy had been succeeded by the Cretan thalassocracy of

Minos, whom Hellenic tradition placed at least three generations before its Trojan Era. The expelled Karian, or Lelegian occupants of the islands had been driven back by Minos into Karia and survived in the adventurous seamen who shared with Ionians in the rediscovery of the outer Mediterranean in the seventh century, B.C. Drs. Köhler and Dümmler identified Karian and Mykenæan, and placed the centre of origin of Mykenæan civilisation in Karia, which was then (1886), still unexplored. Recent investigations on the Karian coast however seem to show that Karia, so far from originating the civilisation in question, remained unreceptive of it until it was already in its decline in Hellas and the Ægean: and that such slight traces as occur are more probably referable to settlements of Ægean (probably early Hellenic refugees) in the catastrophic period which closes the Mykenæan Age.

53. The discovery in Upper Phrygia of rock-cut tombs of early date, with heraldic representations of lions and other figures in archaic style above the entrance, gave ground for a theory of a Phrygian, or partly Phrygian origin for Mykenæan art: especially in view of certain traditional and ethnographic correspondences between Phrygia and early Crete. The dominant race in Phrygia in the eighth and succeeding centuries was regarded, and with some probability, by its contemporaries as of near kindred with one of the strains of the composite Hellenic race. But the most allied strain seems—though this is not clear—to be of late arrival in Hellas; and it has also been shown by more careful examination, and on fuller evidence than at first, that the Phrygian sculptured tombs are of date not earlier than the eighth century, and in many cases appreciably later; so that the evidence points, as in the case of Karia, the other way if at all.

54. Another theory of an origin of Ægean civilisation in Western Asia Minor is that of Dr. Montelius, who regards both the Mykenæan and the Etruscan culture and peoples as representatives of a great emigration westwards from the hinterland of Lydia and Karia. This view lays extreme emphasis on certain Syro-Kappadokian (Hittite)

and Assyrian influences which have been detected in the Mykenæan art, and on a body of Hellenic tradition which represented the Etruscans (Tyrrhenians) who in any case appear to have reached their Italian home by sea, as ethnologically related to the Lydians. Excessive stress also is perhaps laid on the points of contact between Mykenæan and Tyrrhenian civilisation. Mykenæan importations, and imitations of them, are not infrequent in Etruscan, as in Sicilian and Venetic tombs; but there is no clear break at the beginning of the Mykenæan series in Italy such as would be required by the theory of a general immigration of the representatives of an Anatolian culture. Moreover, the complete absence (with the exception of Hissarlik) of Mykenæan city-sites, and the excessive rarity even of single Mykenæan objects on the Anatolian coast, are as fatal to this as to the Karian theory already mentioned.

55. Yet another variant of the Anatolian theory makes Mykenæan art the product of early Hellenic settlers on the Asia Minor coast, under Phœnician and Orientalising influences. This view depends upon the assumption (*SCIENCE PROGRESS*, 1896, p. 353) that Mykenæan civilisation lasted on, even if it did not wholly arise, in the centuries after, rather than before the tenth; and stands or falls with that chronological theory. It is a further obstacle to its acceptance, that, as already noticed, Mykenæan objects have at all events hitherto eluded observation on the sites of all the Anatolian towns, and appears to be confined, on this side of the Ægean, to the islands which fringe the coast; again with the exception of the late or sub-Mykenæan objects which have been found in half-barbarous native tombs at Assarlik, Mylasa, Tschangli and in the neighbourhood of Sardis.

56. A Phœnician theory of the origin of Mykenæan civilisation was of course early in the field. Hellenic tradition had accepted a Phœnician thalassocracy over almost the whole Mediterranean at a period subsequent to that of Minos; an extensive Phœnician commerce in the Ægean as elsewhere, and Phœnician settlements on many points of the Ægean coast: and the immature philology,

archæology and comparative mythology of the first half of the present century found reason to extend the range of Phœnician influences even beyond the very wide limits of the Hellenic view.

57. In spite of the almost complete absence of data for the history of Phœnicia and Phœnician art before the eighth century, B.C., which still confronts us; of the almost uniform divergence of the style of such objects as can be identified as early Phœnician from that of their Mykenæan counterparts, and of the growing mass of evidence that like Karia, Phœnicia, and the whole Palestinian area, borrowed more than it lent, from this, the only living school of culture in the Mediterranean during the period in question, repeated attempts have been made to show that Mykenæ, Ialysos and Gnosso represent, not native Ægean centres, but trading stations or colonial settlements of Semitic adventurers and emigrants established amongst relatively barbarous aborigines.

58. The theory if fully developed would be in a position to take into account the existence of earlier stages of development within the Ægean itself, for Hellenic tradition maintained that these and other islands received Phœnician or "Kadmeian" colonies at a very early date. But it has been most recently treated, by an authority high in another department, in the light of a half knowledge which would seem to ignore these earlier stages altogether, and to treat fully developed Mykenæan art as a newly arrived importation, the earlier stages of which would still be to be sought elsewhere than where they had actually been found ten years previously. The same essay touches also the Etruscan question, and treats Mykenæan imports in the West as further evidence of the œcumenical range of Phœnician commerce.

59. The argument that, as all known Phœnician objects are subsequent to the probable date of the Mykenæan age, they cannot well be the prototypes of Mykenæan art has been ingeniously met by MM. Pottier and Helbig by claiming Mykenæan art as being itself, either wholly, or at least as regards its masterpieces, the long-lost "Sidonian"

art of an earlier Phœnician period known to Greek tradition, but hitherto unrepresented either by monuments in Phœnicia itself, or by undisputed identifications elsewhere ; for the identification of the Keftiu of Egyptian monuments with the Phœnicians is open to grave objections ; and even if it be accepted, it still remains to find any trace of anything but the latest Mykenæan decadence on the Syrian coast ; any proof, that is, that Mykenæan, not to mention Cycladic, objects were ever manufactured by the Phœnicians or their neighbours, or that the offerings of the Keftiu are not themselves rare and valuable importations from the Ægean art-centre.

60. The argument, too, from the voluminous Hellenic tradition of a Phœnician thalassocracy would itself fall to the ground if the very probable hypothesis became demonstrable, that Hellenic anthropology also had laboured under a *Mirage orientale* ; and that the un-Hellenic and pre-Hellenic monuments and other objects which came to light in Hellenic times were interpreted as products of an immediately prehistoric stage of the commercial system which historically prevailed until it was superseded by Hellenic enterprise. Nothing is more striking, in fact, in the speculations of Hellenes, from the sixth century onwards, on their own origins, than their inclination to refer every advance in every department of their civilisation to foreign initiative ; and it has to be remembered that, as above stated, Phœnician trade may be admitted to have been predominant in many parts of the Mediterranean in the blank period which intervenes between the Mykenæan and the Hellenic thalassocracies ; and that consequently it was actually through Phœnician mediation largely that the earlier Hellenic antiquaries obtained from Egypt and Mesopotamia the material for comparison with their own prehistoric monuments.

61. Closely allied to the Phœnician theory is the Syrian or Syro-Kappadokian theory of Dr. Beloch, which would associate Mykenæan art with that style which it is still convenient to label as Hittite. The points of similarity, however, hardly justify the very wide inferences which have

been made from them ; and are themselves in many cases quite explicable as independent loans from Egypt. Here, too, in the present state of the chronological evidence it must be left at least an open question whether the Mykenæan is not the prior member of the comparison.

62. Finally, in discussing certain features in Hellenic religion—one of the most complex religious systems of the world—M. Foucart has collected our fragmentary knowledge of Egyptian naval enterprise, with the object of showing that native Egyptian enterprise in the Ægean, independent of Phœnician trade, is admissible as a means of communication with the primitive Ægean ; but, between proving too much and proving too little, the attempt cannot be said to have wholly succeeded.

63. In complete contrast with all the preceding theories, is the alternative view that the Ægean civilisation is in the main indigenous, and that such Anatolian, Phœnician, or Egyptian features as can be recognised are due not to the predominance of Oriental peoples or trade in the Ægean, but to the expansion of Ægean enterprise into the Levant. Even Hellenic tradition, so apt, as we have just seen, to under-estimate the originality of the civilisation of Hellas, bore witness to frequent marauding and trading visits of “ Achæan ” adventurers to the coasts of Phœnicia, Egypt and Libya : and the repeated occurrence of Ægean imports on Egyptian sites, especially of twelfth and eighteenth Dynasty dates, the contemporary Egyptian record (already referred to) of invasions of Egypt by “ peoples of the isles of the sea ” from the north-west and often apparently with Ægean names ; and the persistence in early Hellenic times of a trade route from the Ægean *viâ* West Crete and the Cyrenaic country to the Nile delta, seem amply to confirm this traditional belief.

64. It is thus, in particular, possible to admit frequent communication with Egyptian civilisation without assuming the existence either of the Phœnician middleman, or of an independent Egyptian trade system : and it will be seen hereafter that an increasing mass of new evidence in regard to the civilisation and the peoples Libya itself has been thus

brought into line, which was practically ignored by the Anatolian and Phœnician theories, and which was in fact for the most part not yet available at the time when these were formulated.

65. Dr. Schliemann's brilliant results at Mykenæ and Tiryns of course gave the impression that the centre of gravity of the Mykenæan world lay on the western side of the Ægean basin, and within the area of the "Kalaurian league" of maritime towns recognised in Hellenic tradition. The settlement in Rhodes being regarded as probably an early colony from Argolis; again in accordance with the legend. Dr. Milchhoefer was the first to direct attention to the importance of the first instalments of very fragmentary Cretan evidence, and the magnificent series of discoveries of Mr. Evans in 1894-6 fully established the view that not only in the decadence and in the culminating period, but also throughout the earlier "Cycladic" stages, Crete exercised an influence more sustained and more dominant than any other section of the Ægean area: and in particular that, as has been noted already, it was in Crete that the pictographic means of written communication which prevailed was mainly elaborated and employed; while it is here too that, thanks to this island's comparative immunity from political and ethnic disturbances, the same script is most nearly approached by the earliest forms of the local Hellenic alphabets.

66. The question of the attribution of Mykenæan art cannot be said to be decisively settled by the evidence hitherto available; though the new material accumulated in the last four or five years in particular has been almost wholly in favour of regarding the Mykenæan objects known to us as products of Ægean industry (the only exception being the discovery in Cyprus of first-class Mykenæan sites whose relations it is not yet possible to estimate with certainty); and very largely also in favour of the theory last mentioned, that the civilisation itself, though undoubtedly affected by its mainly active and not passive contact with Egyptian, and to a less degree with Chaldæan civilisation, assimilated in a remarkable manner the motives and modes

of treatment which it borrowed, and that it can be shown to have developed by a spontaneous and uninterrupted growth, among a group of closely interrelated and indigenous populations.

67. The further question now arises, what is the ethnological position of the Ægean population themselves, and how far the recent anthropological and archæological evidence affects the validity both of the Hellenic tradition, and of certain ethnographic conclusions hitherto commonly accepted?

J. L. MYRES.

(To be continued.)

METAMORPHOSIS IN PLANTS.¹

WHEN I was invited to take part in these College lectures, it was suggested to me that I might advantageously take for my subject the Doctrine of Metamorphosis as illustrated by plants.² I have gladly adopted this suggestion, not only because I hope to interest you by a discussion of the methods and concepts of morphological Botany, but also because there is a certain appropriateness in addressing such an audience as this upon a subject to the development of which at least two distinguished philosophers and men of letters have largely contributed.

The questions which naturally arise at the outset are, What is the Doctrine of Metamorphosis? and To whom are we indebted for it? I think that I may well begin my answer to the first question by giving you a very brief sketch of the general morphology of a plant. You know from your own observation, and you may see by means of the diagrams and specimens now before you, that the body of the more familiar higher plants consists, speaking generally, of certain distinct and easily recognised parts, root, stem, leaves, flowers. The stem, or its branches, bears the leaves and the flowers; the root bears neither. Confining our attention to the flowerless or vegetative region of the

¹ This lecture was delivered to members of Magdalen College, Oxford, on 30th October, 1897.

² The following are some of the more important general works dealing with this subject, of which use has been made in preparing this lecture:—

Wigand, *Kritik und Geschichte der Lehre von der Metamorphose der Pflanze*, Leipzig, 1846.

Whewell, *History of the Inductive Sciences*, iii., 1857 (3rd ed.).

Herbert Spencer, *The Principles of Biology*, 1864-67.

Kirchhoff, *Die Idee der Pflanzen-Metamorphose bei Wolff und bei Goethe*, Berlin, 1867.

Lewes, *Life of Goethe* (3rd ed.), 1875.

Sachs, *Geschichte der Botanik*, 1875 (Engl. ed. 1890).

Goebel, "Vergleichende Entwicklungsgeschichte der Pflanzenorgane," in Schenk's *Handbuch der Botanik*, iii., 1883.

stem, we recognise a definite contrast between stem and leaf: the stem is axial in nature, the leaves lateral and appendicular. Turning now to the flower, we perceive that, unlike the leaf, it is not borne laterally upon the stem, but terminally on a branch. An analysis of the flower shows that it itself consists of an axial and of appendicular parts, the latter presenting widely different forms which are distinguished as the sepals forming the calyx, the petals forming the corolla, the stamens forming the androecium, and, finally, the carpels forming the gynæceum or pistil. The flower is, in fact, a shoot characterised by its limited apical growth, by its undeveloped internodes, and by the reproductive organs which it bears.

We are now in possession of a sufficient body of facts to enable us to proceed with an exposition of Metamorphosis. But before doing so I must point out that this term has not always been used in one and the same sense. It was first introduced into botanical language by Linnæus,¹ but he himself applied it to various phenomena; at one time to the flowering of plants, in which he finds an analogy with the change of the chrysalis of an insect into the butterfly; at another to the production of varieties and monstrosities; again, to the development of a plant from a seed or from a bulb; and, finally, he applies it (in the *Philosophia Botanica*), though vaguely, somewhat in the sense in which we are now about to consider it. It was not until forty years after its first use by Linnæus that the application of the term was clearly limited; and then, not by a botanist, but by a poet—Goethe, in the following words:² “ (§ 1) Every one who in any degree observes the growth of plants will readily notice that certain of their external parts frequently show a change, assuming either entirely or to a greater or less degree the form of adjacent parts. (§ 2) For instance, the simple flower becomes a double one, petals developing in the place of stamens and

¹ *Systema Naturæ*, ii., 1735; *Philosophia Botanica*, 1751 (p. 301 in Willdenow's edition, 1790; p. 355 in Quesné's French edition of 1788); “Metamorphosis Plantarum” (Dahlberg) 1756, in *Amoen. Acad.*, iv.

² *Versuch die Metamorphose der Pflanzen zu erklären*, Gotha, 1790.

anthers; these petals are either exactly like those already present, or they retain visible signs of their origin. (§ 4) The hidden relationship between the various external parts of the plant, such as the leaves, the calyx, the corolla, the stamens, which develop one after the other and, as it were, out of one another, has long been recognised by investigators, and has indeed been specially studied; and the operation by which one and the same organ assumes various forms has been termed the Metamorphosis of Plants. (§ 119) As we have sought to explain all the apparently different organs of the budding and blossoming plant from a single one, namely, the leaf which usually develops at each node; so we have also ventured to derive from the leaf-form those fruits which firmly enclose their seed."

In these words the doctrine of Metamorphosis, as regards the appendicular organs of the plant, is clearly stated: all these parts are regarded as equivalent, whatever their function or their external form. Postponing for the moment the discussion of this generalisation, we may pause to consider the method by which it was arrived at, as also the labours of other workers in the same field.

In addition to Goethe's, there are two names which must always be mentioned when the subject of Metamorphosis is under discussion, the names of Linnæus and of Wolff. With regard to Linnæus, Goethe recognises in him one who had gone far along the same road as himself, but who had failed to attain so definite a goal. Linnæus, it must be admitted, had the sense of the equivalence of the different appendicular organs, but he failed to express it in the clear philosophic form which is the characteristic of Goethe's exposition: with him the idea was implicit, not explicit. The first suggestion of it is to be found in his *Systema Naturæ*,¹ where the following passages occur: "Prolepsis sistit Metamorphoseos Plantarum mysterium, quo Herbæ *Larva* mutetur in Declaratam Fructificationem. . . . Florem dum producat Arbor, Natura anticipabit quinque annorum progenies, simul tum prodituras, formando e foliis gemmaceis

¹ *Systema Naturæ*, ii., 1735; I quote from the thirteenth edition, 1770, p. 8.

futuri anni *Bracteas*, sequentis *Calycem*, insequentis *Corollam*, consequentis *Stamina*, subsequentis *Pistillum*, refertum Medulla granulata *Seminum*, termino vitæ vegetantis." Then again the appendix to his *Philosophia Botanica*,¹ entitled "Metamorphosis vegetabilis" contains the following aphorism: "Principium florum et foliorum idem est". The full bearing of these passages can, however, only be appreciated when his theory of "Prolepsis," or anticipation, is understood. This theory is set out at length in two dissertations, both bearing the title "Prolepsis Plantarum,"² but the earlier one is the more important. The starting-point of the theory seems to have been the observation that when a tree or shrub is planted in a pot, it flowers and fruits annually; whereas if planted in open ground, it produces branches and leaves abundantly, but no fruit. Hence Linnæus infers that in the latter case leaves are produced in the place of the flowers in the former: and conversely, when such a plant is transferred from the open ground into a pot, flowers are produced in the place of leaves. He concludes that in the production of a flower there is an anticipation (*prolepsis*) of what would represent several years growth of vegetative shoots. The way in which he works out this idea, and forms his theory, will be made sufficiently clear by the following headings taken from the earlier dissertation: "Soboles præsentis anni *Folia* esse patet per se; Soboles insequentis anni *Bracteas* esse patet ex *Ornithogalis*; Soboles tertii anni est *Perianthium* (Calyx), quod patet ex *Luxuriantibus*; Soboles quarti anni *Petala*, quod patet ex *Proliferis*; Soboles quinti anni *Stamina* esse, patet ex *Plenis*; *Pistillum* staminibus exhaustis, esse ultimi anni folia a *Plenis* et *Carduis*." This idea, crude as it is, involves the assumption that the organs assigned to each successive year are equivalent; and Linnæus frequently designates them by the common term "*folium*".

It may seem strange that, having gone so far, Linnæus

¹ *Philosophia Botanica*, 1751; third edition, 1790, p. 301.

² "Prolepsis Plantarum" (Ullmark), 1760, in *Amoen. Acad.* vi.; "Prolepsis Plantarum" (Ferber) 1763, *ibid.*

should have failed to come to a full apprehension of the truth ; but his failure may be easily accounted for. In the first place he was hampered by the prevalent fantastic theory of evolutionary development which had originated with Cæsalpini :¹ and in the second place his mind was doubtless so preoccupied with what has turned out to be the altogether unimportant theory of Prolepsis, that he was unable to perceive the great fundamental principle which lay exposed before him.

We pass now to Wolff. Though an animal physiologist and anatomist, Wolff turned his attention to plants, and with remarkable results, in the course of his investigations in support of his theory of Epigenesis by which he controverted the prevalent theory of evolutionary development. For the first time since Malpighi,² the minute structure and more especially the development of the organs of plants were carefully studied. As the chief result of his investigations, Wolff³ made the important discovery that the "appendicular organs," as he termed the leaves, are developed at the growing-point, *punctum vegetationis* he called it, of the stem ; and that this is true whether they be ordinary leaves, as he observed in the common Cabbage, or whether they be parts of the flower, as he observed in the Bean (*Vicia Faba*). Seeing, then, that all these appendicular organs have the same mode of origin, he concludes as follows :⁴ "In the entire plant, whose parts we wonder at as being, at the first glance, so extraordinarily diverse, I finally perceive, after mature consideration, and recognise nothing beyond leaves and stem (for the root may be regarded as a stem). Consequently all parts of the plant, except the stem, are modified leaves."

With this quotation before us, no other conclusion can be drawn than that Wolff had anticipated Goethe's discovery in all essential points, though his exposition of it is

¹ *De Plantis*, 1583.

² Malpighi, *Anatomes Plantarum Idea*, 1675 ; as also other works in his *Opera omnia*, Lond. 1686 ; Lugd. Batav., 1687.

³ *Theoria Generationis*, 1759 ; *Theorie von der Generation*, 1764.

⁴ "De Formatione Intestinorum," *Nov. Comment. Acad. Petrop.*, xii., 1767.

less formally complete than that of Goethe. But this fact does not in the least detract from Goethe's merit ; for, in spite of insinuations to the contrary, there is every reason to believe that Goethe had not seen the *Theoria Generationis* when he wrote his *Versuch*. When he did become acquainted with Wolff's work, he did not fail to recognise its importance, and spoke of its author as his "admirable precursor". It is extremely interesting to observe that these two men of distinguished ability, working independently, arrived at essentially the same conclusions by what I venture to regard as quite opposite methods. As to the nature of Wolff's method, there cannot be the slightest doubt. He approached the study of plants, it is true, for the purpose of establishing his theory of Epigenesis, but not with any preconceived ideas bearing upon Metamorphosis. He made his observations, and from them he drew his conclusions. But with Goethe the case seems to have been different. It is true that he made observations, but these observations would appear to have served rather to confirm a theory already formed in his mind than to suggest one. Whilst the method of Wolff was purely inductive, that of Goethe was essentially deductive. This is perhaps a hard saying, the more so as Goethe himself distinctly repudiates the rôle of an *à priori* philosopher.¹ "It is by no sudden and unexpected inspiration of genius," he writes, "but through long prosecuted studies that I have arrived at my results," a position which is even more definitely asserted in his first conversation with Schiller. Here is Lewes' account of it : "One day, in May 1794, they met, coming from a lecture given by Batsch at the Natural History Society in Jena ; in talking over the matter, Goethe,

¹ In another passage, however, he seems to admit the deductive character of his work : "I saw that a whole life of talent and labour was requisite to enable any one to arrange the infinitely copious organic forms of a single kingdom of nature. Yet I felt that for me there must be another way, analogous to the rest of my habits. The appearance of the changes, round and round, of organic creatures had taken strong hold on my mind. Imagination and Nature appeared to me to vie with each other which could go on most boldly and most consistently." (*Zur Morphologie*, 1817, i., p. 30).

with pleased surprise, heard Schiller criticise the fragmentary method which teachers of science uniformly adopted. When they arrived at Schiller's house, Goethe went in with him, expounding the theory of Metamorphosis with great warmth. Taking up a pen he made a rapid sketch of the typical plant. Schiller listened with great attention, seizing each point clearly and rapidly, but shaking his head at last, and saying: 'This is not an observation, it is an Idea'. Goethe adds: 'My surprise was painful, for these words clearly indicated the point which separated us. The opinions he had expressed in his essay on *Anmuth und Würde* recurred to me, and my old repulsion was nearly revived. But I mastered myself, and answered that I was delighted to find I had ideas without knowing it, and to be able to contemplate them with my own eyes.' There can be no question of Schiller having been in the right. . . . The typical plant, Goethe knew very well, was not to be found in nature; but he thought it was *revealed* in plants. Because he arrived at the belief in a type through direct observation and comparison, and not through *à priori* deduction, he maintained that this type was a perception (*Anschaung*), not an idea." In another place (p. 345) Lewes expresses the same view in his appreciation of Goethe as a man of science: "Goethe was a thinker in science, a manipulator of scientific ideas. He was not one of those laborious and meritorious workers who with microscope and scalpel painfully collect the materials from which Science emerges. He worked, too, in his way, and everywhere sought in the order of nature for verification of the ideas which he had conceived *à priori*. Do not, however, mistake him for a metaphysician. He was a positive thinker on the *à priori* method; a method vicious only when the seeker rests contented with his own assumptions, or seeks only a partial hasty confrontation with facts—which Bacon calls *notiones temerè a rebus abstractas*; a method eminently philosophic when it merely *goes before* the facts, anticipating what will be the tardy conclusions of experience."

Moreover, it may be pointed out that the evidence upon which Goethe relied was insufficient to warrant the generali-

sation at which he arrived. For what is this evidence? It consists, in the first place, of a careful comparative study of the appendicular organs, noting the various forms which they assume in different parts of any one plant as well as in different plants, with special reference to those cases, to be found here and there, where transitional forms occur. But external resemblances are by no means conclusive; it would be easy to cite numerous examples of close external similarity between organs as distinct as stems and roots, stems and leaves, and roots and leaves; in fact several examples of the kind are now before you. Then again he relies upon monstrosities, such, for example, as stamens which have assumed the character of petals, as sepals that of ordinary green leaves. But teratological evidence is quite inconclusive, though it is suggestive; for when any part deviates in form from the normal, it does not follow that the form which it assumes is a primitive form indicative of its essential nature; there is no conclusive reason for regarding such a case as one of reversion. What was lacking in Goethe's observations was largely supplied by Wolff's elaborate developmental studies; for the study of development is the only sure means of determining the nature, and thus also of establishing the homologies, of the parts of any living organism.

In reviewing the doctrine of metamorphosis in plants as it was left by Wolff and by Goethe, we cannot fail to perceive that, great as was the advance they had made, it was far from complete. In the first place, its application was confined to one set of organs, the appendicular organs, neglecting others of equal importance, such as the stem and the root. And even with regard to the appendicular organs much was left to be desired. They tell us that all these organs are "leaves," and that the character common to them is their mode of development from the growing-point of the stem. They further tell us that the organs of the flower are "modified leaves". But the question remains, of what leaf or leaves are they the modifications? To this question two alternative answers have been offered. The one which we may distinguish as the concrete,

suggests a material metamorphosis of one kind of leaf into the other ; for instance, on this view a sepal might be regarded as a metamorphosed foliage-leaf, a petal as a metamorphosed sepal, a stamen as a metamorphosed petal, and so on. The result of this process would be that the foliage-leaf would come to be regarded as the primitive typical form of leaf to which all the others might be traced back ; a view which became discredited in consequence of the extravagant application of it by the school of “natural philosophers” which Goethe’s *Versuch* may be said to have called into existence ; whose members, following in the footsteps of Goethe rather than of Wolff, but without Goethe’s genius, speculated instead of observing. Then there is the other answer, the abstract or transcendental answer, that the various forms of leaves are modifications not of any existing form, but of some imaginary typical leaf. It would appear that this latter answer is the one which Goethe offered, though he is not altogether clear on the point. Having, in § 119 of the *Versuch*, pointed out that all the various organs of the flowering plant may be referred to one, the leaf, he proceeds in § 120 as follows : “It is obvious that we ought to have a general term by which to designate the organ which is metamorphosed into so many forms ; for the present we must accustom ourselves to realise that these phenomena take place backwards and forwards. For we can as well say a stamen is a contracted petal as we may say of the petal that it is an expanded stamen ; or that a sepal is contracted foliage-leaf, as that a foliage-leaf is an expanded sepal.” Evidently, whilst he had arrived at the extension of the term “leaf,” he was unable to formulate the intension of the term. What he sought was the morphological concept of the leaf ; and the reason why he failed to form it was that the morphological Botany of his time was too superficial and too physiological to admit of such conception.

But now I must give you some idea of what I mean by a morphological conception of the plant, or of part of it. You may perhaps have noticed that so far I have uniformly spoken of the various parts of plants as “organs,” and I

have done so because this was the terminology of the period in the history of Botany which we are considering. This mode of regarding them had come down from Aristotle,¹ who defines “the parts of plants as organs,² though quite simple ones; thus the leaf is the covering of the pericarp, the pericarp of the fruit; the roots are the analogues of a mouth, for both absorb nourishment”: and again,³ “the nature of plants being stable, there are not many kinds of heteromerous parts; in the few kinds of work, there is use for but few organs; wherefore they are to be considered with regard to their form”. That is, all the parts of plants were considered primarily from the physiological point of view of their function; analogies had been drawn between them, but not homologies. Some indication of a morphological conception of them—that is, of their nature apart from their function—is to be found in the writings of Theophrastus of Eresus;⁴ but no real progress was made in this direction until late in the seventeenth century, when Joachim Jung, a philosopher, sometime Professor of Logic, Physics and Metaphysics, and ultimately Rector of the Johanneum in Hamburg, wrote his remarkable botanical treatises,⁵ in

¹ Aristotle's work on plants (*θεωρία περὶ φυτῶν*) is lost, though a treatise, *De Plantis*, is generally included in editions of his works, of which treatise Whewell says (*Hist. Induct. Sciences*, iii.) that it is “an imposture of the middle ages, full of errors and absurdities”: but his extant writings contain numerous references to plants: all these passages have been collected by Wimmer under the title “*Phytologiæ Aristotelicæ Fragmenta*” (1838), of which Meyer gives a translation in the first volume of his *Geschichte der Botanik*, 1854.

² *De Anima*, ii., cap. i.

³ *De Partibus Animalium*, ii., cap. x.

⁴ *Historia Plantarum*, lib. i., cap. ii.

⁵ They were not published until after his death, when they were issued by his pupils, Fogel and Vaquetius. The most important is the *Isagoge Phytoscopica*, 1678. Some of his definitions are of sufficient interest to warrant quotation; I quote from Albrecht's edition of the collected works (*Opuscula Botanico-Physica*, Coburg, 1747).

“*Planta proprie dicta duabus constat partibus, radice et superficie sive parte superna.*”

“*Radix est pars inferior quæ intra corpus solidius quod plantæ sedem præbet, abdita, et alimento attrahendo destinata est. Pars superna est,*

which he reveals striking morphological insight. Whilst his definitions may be criticised as lacking in precision, it must be admitted that he had grasped the fundamental ideas of morphology. Analysing the plant-body into root and shoot, and the shoot into stem and leaf, he defines these parts, not according to their function, but according to their relative position and their form; that is, he defines them not as "organs," but as "members" of the body.¹

As in the case of Wolff and of Goethe, so also in that of Jung, the publication of new and epoch-making views produced no effect upon the contemporary science, beyond the fact that Jung's work was incorporated by Ray in his *Historia Plantarum* (1686); nor, in Jung's case, was the neglect of his contemporaries atoned for by the recognition of posterity.² Certainly there is no trace of his influence

quæ supra sedem plantæ in liquido corpore (aëre vel aqua) exstat, et speciei eminus propagandæ præcipue inservit."

"*Limes communis, in quo duæ istæ partes cohærent Fundus plantæ dicitur, Græcis πυθμήν.*"

"Pars superna, aut pars partis supernæ est vel *Caulis*, vel *Folium*, vel *Flos*, vel *Fructus*, vel minus secundaria aliqua pars, v.g. *Villus* aut *Spina*."

"*Caulis* est pars superna, in altitudinem ita exporrecta, ut anteriora a posterioribus, vel dextra sinistris non differant."

"*Folium* est, quod a sede, cui adhæret, ita in altitudinem, sive longitudinem, et latitudinem extenditur, ut tertiæ dimensionis termini inter se differant, h. e. superficies folii interna ab externa."

"Superficies folii interna, quæ et *superior*, item *supina* dicitur, est quæ caulem respicit, ideoque vel cavitatis aliquid obtinet, vel minus convexa est, quam altera *exterior* sive *inferior*, sive *prona* superficies."

"*Perfecta planta* vel caulem a folio distinctum gerit vel confusum cum eo. Quæ confusum gerit, ea est, quæ folium e folio producit, atque, ita ex meris foliis caulem quasi quendam ramosum componit, qui eo *discrepat a natura* caulis, quod partes prismatis figuram non referunt, sed crassitiem latitudine minorem habent *h.e. in latitudinem expansæ* sunt, ideoque etiam *folia* dicuntur. Rursus in eo *caulis indolem exprimit* hoc folium, quia *se propagat*, quod folio proprie dicto non competit. Differt etiam id, quod folium dicitur in his plantis a folio proprie dicto, quod *superficies* latiores, figura non diversas, *h.e. interiorem ac exteriore non differentem* obtinet."

¹ See Hofmeister, *Allgemeine Morphologie*, 1868, p. 409, footnote 2; Hanstein, *Bot. Abhandlungen*, i., 1870, p. 92.

² Albrecht, in his preface to the collected botanical works of Jung, says on this point: "Mirati sumus sæpius, qui factum sit, ut in historicis

in the works of Linnæus, any more than in those of Wolff and of Goethe. But Wolff's own work contributed materially to the advance of morphology. As already pointed out, he established a definite contrast between the stem as the axial member of the body, and the leaves as the appendicular members. Whilst it is true that he regarded the root as of the same nature as the stem, it is clear that he did not do so in consequence of any confusion of ideas; what he meant to convey was that they are to this extent of the same nature, that they are both axial, as distinguished from appendicular, members. Far from confusing them, he was the first to point out one of their fundamental morphological differences, namely, that the stem bears leaves whilst the root never does so; and further, he discovered the endogenous mode of development which is characteristic of the root.

If we now compare the definitions of the chief members of the plant given by Aristotle, Linnæus and Wolff, the essential difference between the physiological and the morphological points of view stand out in striking contrast. Taking the root, for example, we find that Aristotle regards it as the organ for the absorption of nourishment and that Linnæus describes it in almost identical terms: "*Radix descendens, aquosa sorbens, nutriens*" (*Syst. Nat.*); while Wolff sees in it the part of the plant which does not bear leaves. At the same time it must be clearly apprehended that these two points of view are not mutually exclusive: for whilst the root is distinguished morphologically as the member of the plant-body which bears neither leaves nor flowers, it may be regarded physiologically, and with equal justice, as an absorbent organ. It is not too much

de fatis et incrementis studii botanici scriptis, multo minus vero in Isagogicis seu Institutionibus nulla fere, vel saltem rarior Jungianis meritis, quæ sane in hac re magna sunt, digna legatur commemoratio, Quin ipse solertissimus et in perfectiorem rei botanicæ emendationem natus Linnæus Doxoscopiarum Jungianarum ne meminit quidem, licet vir sit in bibliothecis botanicis versatissimus, atque Isagoges fecerit justam mentionem. Causam sane aliam suspicari non licet, nisi quod ad oculos tanti viri illæ non pervenerint, alias enim integerrimum virum Jungium nostrum, nisi in critica botanica socium sibi adjunxisse, certe tamen inter Philosophos æque primum locum illi assignaturum fuisse non dubitamus."

to say that one of the main obstacles to the advance of morphological Botany has been the failure to recognise the distinctness of these two conceptions; the confusion of what any part of the plant *is* with what it *does*.

But I am anticipating somewhat. Before I can properly develop the foregoing ideas, we must trace the onward progress of morphology since the time of Goethe.

As I have already indicated, the period immediately succeeding the publication of Goethe's *Versuch*, up to nearly the middle of the present century, was one fruitful in little else than wild theorising; it was the period of the so-called "natural philosophers". This fortunately culminated in a reaction to investigation and induction. On a sudden, as it were, a band of men arose, of brilliant ability and indefatigable industry, whose great achievements have revolutionised not only the department of morphology, but the other branches of Botany as well; I need only mention the names of Schleiden, von Mohl, Naegeli, Hofmeister, Robert Brown, Irmisch, Hanstein, Alex. Braun. It would take me too long to give you anything like a detailed account of their researches; to do so would be to write at length one of the most important and striking chapters in the history of Botany. All that I can hope to do is to lay before you some of the results of this remarkable renaissance of our science.

I need hardly say that the morphological distinction of the three chief members of the plant as established by Wolff, root, stem, leaf, has been fully confirmed, and has been crystallised in the abstract terms *caulome* and *phyllome*¹ for stem and leaf; the similar terms *trichome* and *thallome* being added to designate, the former the epidermal members of the body (hairs, etc.), the latter the undifferentiated body, characteristic of the lower plants, which presents no distinction of the three primary members. Another important result has been the extension of the idea of meta-

¹ Naegeli und Schwendener, *Das Mikroskop*, 1867. The correlative term *rhizome* for the root was not introduced, probably because the word was already in use in Botany, being applied to a creeping underground stem.

morphosis, which had hitherto been confined to the leaves, to the other members. For instance, we now know that a tuber, such as that of a potato, or of a Jerusalem artichoke (*Helianthus tuberosus*), is not a root, though it is subterranean, but is a metamorphosed branch; for if the young shoots at the lower part of the potato-stem be not covered up with earth, as is done in the ordinary course of cultivation, they develop into green leafy branches; whereas, when buried in the soil, they develop into potatoes. Again, we now know that such things as turnips and carrots are really metamorphosed roots.

But with this extension of the scope of metamorphosis, important knowledge has been gained as to its limits. It may be now laid down as a general law that metamorphosis can only take place within each category of members, and not from the one to the other;¹ that, for instance, a leaf cannot be metamorphosed into a stem, or a stem into a leaf, or either of them into a root. At the same time it frequently happens that one member may discharge the functions of another, thus becoming analogous to it; but in doing so it does not change its morphological nature and become homologous with the member whose work it has taken over. For instance, I would draw your attention to the Cactus (*Opuntia*) on the table before you. The flattened lobes of which its shoot consist somewhat resemble leaves in form, and actually perform the functions of leaves, yet morphologically they are not leaves, but leaf-like segments of the stem, the plant being leafless. The same thing is even more strikingly shown by this branch of the Butcher's Broom (*Ruscus*) where the stem, which is of normal form, bears what look like leaves, but are really leaf-like branches (*phylloclades*). Again, one part of a member may assume the form and discharge the functions of another. Here is a branch of an Australian Acacia in which the stems bear organs resembling leaf-blades; they are, however, not leaf-

¹ It must, however, be admitted that exceptions do occur. Thus the conversion of the root into a shoot has been observed in *Neottia Nidus-avis* (see Hofmeister, *Allgemeine Morphologie*, 1868, p. 428), and in *Anthurium longifolium* (see Goebel, in *Bot. Zeitg.*, 1878).

blades but leaf-stalks (*petioles*), which simulate the leaf-blade and discharge its functions (*phyllodes*); in this plant the leaf-blade, which is usually the important organ of nutrition, is absent. Rootless plants offer good examples of the assumption of a root-like form and function either by underground stems (*Psilotum*, saprophytic orchids such as *Neottia*, *Corallorhiza*, *Epipogon*), or by leaves (*Salvinia*). It would be easy but useless to multiply examples.

We may now proceed to inquire what is the attitude of modern morphology towards the doctrine of metamorphosis. You will remember that I have mentioned the two possible answers, the one concrete, the other abstract, to the important question, what is it that undergoes metamorphosis? Are we to regard all the various forms of leaves, for instance, as modifications of an ideal "type"—of an "*Urblatt*"—or of some actually existing form? Let us see what is the verdict brought in by actual research.

The point is one which has been much discussed. It is not unnatural that some of the earlier writers, whilst still under the influences of Goethe and his immediate followers, should have accepted the abstract view. Thus we find Alex. Braun writing:¹ "This ideal ladder which Goethe perceived in the metamorphosis of plants, is a speaking testimony of the profound conception of it which he had formed; for that which leads the formative process of the plant from one rung to the next, which connects the rungs into a ladder, which reveals each rung, though distinct from its predecessors, as a product of their modification, cannot be other than inward and ideal". Moreover in a footnote he says further: "That Goethe was not altogether free from the erroneous notion that one organ of a plant might be actually transformed into another, for instance, stamens into petals, or pistils into leaves, is evident from the very first paragraph of his introduction". The same position has also been taken up by some later writers. Thus Schmitz² says: "After all, stamens and foliage-leaves

¹ "A. Braun, *Verjüngung in der Natur*," 1851: p. 61 of the English translation, *Botanical and Physiological Memoirs*, Ray Society, 1853.

² Schmitz, "Die Bluthen-Entwicklung der Piperaceen," in Hanstein's *Bot. Abhandl.*, ii., 1872, p. 32, footnote.

are merely phyllomes which are differentiated, having assumed different forms in relation to their diverse functions : but stamens are by no means metamorphosed foliage-leaves. Stamens and foliage-leaves are only equivalent, as regards their relation to the entire shoot, to the axis which bears them." And again Hanstein,¹ in one of his last memoirs, writes : "Further, the fact that all these various forms of leaves succeed each other from below upwards on the shoot, and are, at the same time, frequently connected by intermediate forms so that the primitive identity of their morphological nature is more clearly brought to light, reveals them as modifications of one and the same organic type which successively transforms itself into each of these organs. This process, which is rather theoretical than actual, is what is termed the Metamorphosis of the leaf."

On the other hand Schleiden,² faithful to the traditions of Wolff, stoutly maintains the view of material metamorphosis. Beginning with the blunt statement that "Science, to her great detriment, received this thought (metamorphosis) not from Wolff but from Goethe, a thought which might have proved so fruitful, but which, owing to the manner in which Goethe introduced it, has been of relatively so little use," Schleiden goes on to give his conception of metamorphosis as "the fact that the plant possesses but few essentially different organs, and that all the others differ only dynamically from these fundamental organs in that there is inherent in them a tendency to undergo a definite and characteristic development and modification ; a tendency which is, however, not so absolute but that it can be overcome under special circumstances, and then the normal form of the organ again becomes apparent". The language of this confession of faith is somewhat involved, but its purport is unmistakable.

There can no longer be any doubt that the weight of

¹ Hanstein, "Beitr. zur allgemeinen Morphologie der Pflanzen," *Bot. Abhandl.*, iv., 1882, p. 30.

² Schleiden, "Einige Blicke auf die Entwicklungs-geschichte des vegetabilischen Organismus bei den Phanerogamen ;" in his *Beiträge zur Botanik*, 1844, p. 86.

accumulated evidence is distinctly in favour of the material view of metamorphosis ; that is, of a genetic relation between the various forms of any one member. To take some simple illustrative cases : it is clear, for instance, that a potato-tuber is produced by the actual metamorphosis of an ordinary branch ; nor is it less clear that a turnip is the product of the actual metamorphosis of an ordinary root. The matter becomes more complicated, when we turn to the leaves, on account of the great variety of form which these members present, and calls therefore for more elaborate treatment. For whereas in the cases of shoot and root just mentioned, only a single metamorphosis takes place ; in the case of the leaves, on the contrary, we have the question of successive metamorphoses to consider, the transformation of one metamorphosed leaf into another.

In order to deal adequately with the metamorphosis of leaves, it will be necessary for me to inflict upon you a brief statement of the different classes or categories of leaves to be met with on the plant, based on the classification proposed by Alex. Braun¹ and by Hanstein.² They distinguish :—

Belonging to the Vegetative Region.

1. The *Cotyledons*, or seed-leaves, the first leaves of the embryo plant.
2. The *Cataphylls*, or scale-leaves, such as form the covering of buds.
3. The *Euphylls*, the ordinary green foliage-leaves.

Belonging to the Reproductive Region.

4. The *Hypsophylls*, including :—
 - (a) The *bracts*, which are intermediate between the foliage-leaves and the floral leaves ; (b) the *sepals*, forming the calyx ; (c) the *petals*, forming the corolla.

¹ Alex. Braun, “ Verjüngung,” Eng. Trans., p. 62.

² Hanstein, *Bot. Abhandlungen*, iv., 1882, p. 28.

5. The *Sporophylls*,¹ the essential reproductive organs, including :—

- (a) The *stamens*, forming the andræcium ; (b) the *carpels*, forming the gynæceum or pistil ; in Flowering-plants.

But this enumeration by no means exhausts the possibilities of leaves, for these various categories may themselves exhibit polymorphy to a remarkable degree. For instance, some of the cataphyllary leaves of the onion, which is nothing but a subterranean bud, become enlarged to serve as depositories of nutriment ; again, the leaves belonging to the category of euphylls present, besides the endless varieties of their typical form, a wide range of modification, assuming forms so diverse as that of spines in the Barberry, of tendrils in the Pea, of tentacular leaves in the Sun-dew, of pitchers in *Nepenthes* and *Sarracenia*.

What now is the evidence to prove material metamorphosis in the case of leaves? When we have to deal with simple metamorphosis, with metamorphosis that is taking place within the limits of any one category of leaves, the evidence is as convincing as that which I have adduced with regard to stems and roots. For instance, no one has ever suggested that the spiny leaf of a Barberry, or the pitched leaf of *Nepenthes*, is anything but a modification of an ordinary foliage-leaf ; or that the nectary of the Monks-hood is anything but a modified petal ; or that the petaloid investment of the inflorescence of the Trumpet-Lily (*Richardia*) is anything but a bract. The most superficial observation would suffice to disprove any contrary assertion. But when we go on to consider the possibility of a genetic relation existing between leaves of different categories, we have a more complex problem to solve. However, some satisfactory evidence on this point is forthcoming ; based to some extent on the actual observation of development, but mainly on the occurrence of intermediate forms. The consideration of the cataphylls affords a good

¹ We owe this term to Schleiden, *Grundzüge der wissenschaftlichen Botanik*, English Edition, 1849, pp. 194, 346.

illustration of the developmental evidence. Prof. Goebel,¹ to whom we owe most of our knowledge on this subject, has shown that, in many cases, the scales protecting the leaf-buds are really foliage-leaves which have undergone modification which consists either in the abortion of the leaf-blade, or in a change of form of the entire leaf. He has ascertained this, not only by the study of development, but also experimentally in a most ingenious way. He has found, namely, that if the leaf-buds formed in any one year to expand in the following year, are forced to expand in the year of their formation,—which may be accomplished either by cutting off the growing end of the branch bearing the buds, or by removing all its leaves—the buds form no bud-scales, but the structures which would have formed bud-scales in the normal course now develop into foliage-leaves instead. With regard to the bracts, the matter is comparatively simple; in many cases they are quite indistinguishable from the foliage-leaves, and in others their relation to them can be traced by a series of intermediate forms. The same kind of evidence applies also to the sepals. In many cases they cannot be distinguished from the bracts; and it must be further borne in mind that, inasmuch as they stand in the same relation to the flower-bud as do the bud-scales to the leaf-bud, there is a certain connection to be traced between them. Coming now to the petals, we find that in many cases (as in the Lily) they cannot be distinguished from the sepals; moreover their peculiar attributes of colour and form may be exhibited, not only by the closely related sepals, but by the more remotely related bracts (*Bougainvillea Pointsettia*). Finally, in the case of the sporophylls, we have to rely upon the manifestation by them of phylloid characters so frequent in the case of monstrous and of double flowers, as well as upon the comparatively few cases in which the petaloid form is the normal (stamens of *Canna* and its allies; petaloid style of *Iris*).

But if this evidence suffice to prove the existence of material metamorphosis in the category of leaves, the

¹ Goebel, *Vergleichende Entwicklungsgeschichte*, p. 248.

questions still remain, is there some primary form of leaf of which all the others may be regarded as modifications? and, if so, what is that form? The first of these questions must, as it seems to me, be answered in the affirmative; for material metamorphosis necessarily involves such an assumption. We have traced a series of forms of leaves, differing the one from the other by the characters which they have assumed as the result of adaptation to their several functions: the cataphylls to protect the buds; the foliage-leaves to discharge certain nutritive functions; the sepals to protect the flower; the petals to attract the visits of insects; the stamens and carpels to bear the reproductive cells; and this series must have had a definite starting-point. In endeavouring to determine this starting-point, to discover this primitive form, it is evident that two alternatives are open to us; the series has two ends which we may distinguish as the higher and the lower, and it is obviously possible to take one or the other of these ends as the starting-point, to regard either the sporophyll or the foliage-leaf as the primitive form. Goethe long ago was struck by the fact that both *ascending* (from the foliage-leaf upwards) and *descending* (from the sporophyll downwards) metamorphoses are exhibited by plants. Which of these modes is the course actually followed in the differentiation of the organs of the plant?

As a matter of fact, both these possible views have their supporters; and the point may be regarded as still open to discussion. There can be no doubt that those who first introduced the idea of material metamorphosis, basing themselves upon the ascertained fact that the leaves are developed in acropetal succession at the growing-point of the stem—so that the youngest leaves are nearest to, the oldest most remote from it—adopted the theory of ascending metamorphosis, regarding the foliage-leaf as the primitive form of which all the others are modifications as the result of special adaptation to their various functions. In the present day the strongest supporter of this view is Goebel, whose position may be gathered from the following quotation: “Herein lies the morphological equivalence of the

leaf-organs, that the plant, as a matter of fact, gives origin to leaves of one kind only, namely the foliage-leaves, the ultimate form of which is modified in various ways by conditions which arise in the course of their subsequent development". Whilst Goebel agrees with his predecessors in regarding the foliage-leaf as the material basis of metamorphosis, it appears that there is a difference between them as to the mode of the process: according to the older view the more highly modified organs are the products of *mediate* metamorphosis of the foliage-leaf; the stamen, for instance, being traceable to it through the petals and sepals: Goebel, on the contrary, asserts that metamorphosis is *immediate*, regarding all other forms of leaves as being directly traceable to the foliage-leaf, though they may be sometimes transformed into one another.

The view that the foliage-leaf is the primitive leaf-member, and that the floral leaves are its derivatives, is then based upon the fact that, as a rule, the vegetative precede the reproductive organs in ontogenesis. The opposite view that the most highly specialised floral leaf, the sporophyll, is the primitive phyllome, is based upon the fact that, phylogenetically, the reproductive precede the vegetative leaves. The full discussion of the latter view would involve an amount of technical detail which would be altogether out of place in such a lecture as this. I must content myself with briefly explaining that in such lowly organised plants as the Mosses (Bryophyta), the sporophyte, that is the form in the life-history which correspond to the Flowering-plant, consists, in the simplest case, of little more than a mass of reproductive cells; and even in the more complex Mosses, this form never presents any differentiation of vegetative organs. We find, then, that in such primitive plants as the Mosses, the reproductive is the predominant function. Turning now to the Fern-like plants (Pteridophyta), the plants which come next above the Mosses, we find that they have well-developed leaves; and further, that in the more primitive forms the leaves are distinctly differentiated into sporophylls and foliage-leaves; whereas in what may be regarded as the secondary forms, this distinction is either

slight or altogether absent. Prof. Bower,¹ who has recently urged with great force the claims of the sporophyll to be regarded as the primitive leaf, sees in the differentiation of leaves in the Ferns, first of all an increase of the reproductive, spore-bearing area, as compared with the Mosses, by the development of sporophylls; and then the further evolution of the nutritive functions by the differentiation of some of the sporophylls as foliage-leaves. The relative positions of Goebel and of Bower may be summed up in a single sentence: whereas Goebel regards a sporophyll as a foliage-leaf which has become metamorphosed in consequence of the development of spores from its substance, Bower regards a foliage-leaf as a sporophyll which has lost its primitive form in consequence of sterilisation. The Fern-like plants are the main battle-ground upon which this question is being contested. When the issue is decided there, it will also have been decided for the Flowering-plants (Phanerogamia); for whatever is true of the relation between the various forms of leaves in the Fern-like plants, must also be true with regard to their descendants the Flowering-plants. It is, therefore, not necessary to attempt a discussion of the matter as regards Flowering-plants: I will merely mention the significant fact that the flowers of the earliest Flowering-plants (Gymnosperms) consist entirely of sporophylls, being altogether destitute of petals and sepals, whence the primitive nature of the sporophyll may be fairly inferred.²

¹ Bower, "A Theory of the Strobilus in Archegoniate Plants," *Annals of Botany*, viii., p. 343, 1894: "Studies in the Morphology of Spore-producing Members—Equisetineæ and Lycopodineæ," *Phil. Trans. Roy. Soc.*, vol. 185 B, 1894, p. 473. Some interesting facts supporting Prof. Bower's view have recently been published by Atkinson, *The Transformation of Sporophyllary to Vegetative Organs*, Boston, 1896. He has found in certain Ferns (*Onoclea sensibilis* and *Onoclea Struthiopteris*) which have clearly differentiated sporophylls and foliage-leaves, that if the young foliage-leaves be destroyed early in the season, the sporophylls (which develop later) do not present their characteristic form, but more or less resemble the foliage-leaves and are partly or completely sterile. These observations show that when nutritive functions are, as it were, forced upon sporophylls, they assume the habit of foliage-leaves.

² This point is well brought out by Mr. Grant Allen in his *Colours of*

We see that, in spite of the differences of opinion as to the precise mode in which it takes place, modern Botany has established the doctrine of material metamorphosis upon a sound basis of observed fact; and in arriving at this result I have accomplished one of the chief objects of this lecture. I will, however, venture to trespass on your patience for a few minutes longer to briefly consider in conclusion some of the theories which have been propounded for the purpose of explaining the occurrence of metamorphosis.

It is easy to account for the various metamorphoses of the members of the plant on the metaphysical principle of final causes, by simply asserting that any given metamorphosis is to the advantage of the plant—for instance, that the metamorphosis which results in the production of petals is to be attributed to the advantage gained by the plant in the cross-pollination by means of insects which the brightly coloured petals ensure—and, no doubt, as far as it goes, this explanation is sound. But I desire to lead you farther than this, and to inquire on the physical method whether or not some efficient cause may be found; and, if so, what the nature of the efficient cause or causes may be.

Some light will be thrown on this difficult matter by the consideration of metamorphoses, which we may distinguish as traumatic, caused by the attacks of insects and other parasites. It is known that chloranth, a condition in which all or the great majority of the floral organs assume the form and colour of foliage-leaves, is frequently induced as the result of puncture by insects or by the growth of fungi in the tissues¹. Here then is a case of metamorphosis which can be clearly traced to an efficient cause; but we must endeavour to penetrate farther into the matter and ascertain as far as may be the mode in which the attacks of these parasites bring about so striking a change.

Flowers, "Nature Series," 1882. His point of view is, however, somewhat different from that stated above, in that he assumes that "The starting-point consists of a plant having three kinds of organs, true foliage-leaves, staminal leaves and carpellary leaves" (p. 14).

¹ Masters, *Vegetable Teratology*, 1869, 279 (Ray Society).

Turning now to those cases of metamorphosis which cannot be traced to injury, we find ourselves at once confronted with the great biological problem of the antagonistic relation existing between growth and reproduction. The fundamental observation illustrating this is that recorded by Linnæus as the basis of his theory of Prolepsis, to which I have already drawn attention—the observation, namely, that when a tree is abundantly supplied with nourishment it develops leafy shoots, whereas when transplanted to poor soil, or to the narrow limits of a pot, it develops flowers. This idea of the relation between deficient nourishment and flowering—that is, reproduction—also occurs in Wolff's writings, for he refers flowering to a *vegetatio languescens et evanescens*. With Goethe the idea takes a somewhat different form. He assumes that the foliage-leaves on the lower parts of the plant are products of coarser nutritive sap which, as it ascends, undergoes refining by filtration and thus becomes suited for the development of the floral organs.

On comparing the views of Linnæus and of Goethe, we observe that they differ in that the former refers metamorphosis of the leaves entirely to external, the latter entirely to internal causes. We will briefly consider this point. Whilst there is no doubt that external conditions may induce, there is evidence to show that they do not originate, metamorphosis. To return to an example already cited: it is true that the lower shoots of the potato-plant will not develop into tubers unless they are covered up with earth; but this treatment does not confer upon them the capacity of developing into tubers, for the shoots of other plants treated in the same way do not so develop. Again, in Linnæus' observation given above, whilst it is true that a certain change in the environment of a tree may hasten its flowering, this change does not confer upon the tree the power of flowering; that it possesses already, and would eventually manifest it even though left to grow in a rich and unlimited soil. And, finally, in the case lately quoted of chloranthly resulting from injury, the puncture of the insect would lead to no such change were not the capacity for it already inherent in the plant.

Whilst then we admit that external conditions and agencies may be the *vera causa* of any given metamorphosis, we cannot fail to perceive that they constitute but the proximate cause, whilst the ultimate cause resides in the organisation of the plant itself. This has long been recognised, and a plastic force or *vis formativa*,¹ whereby it develops the characteristic form of its organs, has been attributed to the living organism. I need hardly point out, however, that such an assumption as this is no explanation of the phenomena; it is merely a statement, in abstract form, of the facts of development. It is obviously true, but it is no more than a truism beyond which we must pass if we would attain to a knowledge of the physical conditions which are the ultimate causes of the phenomena. The only attempt in this direction with which I am acquainted is that of von Sachs²—whose recent loss all Botanists have to deplore—who has suggested that the development of the different kinds of organs is the result of the elaboration of certain specific plastic materials in the plant, a view to which Goebel also adheres to in the main.³ But this suggestion has not been sufficiently worked out to warrant its acceptance. And even were it proved to be true, the *vis formativa* would not have been explained away, for the questions would still remain: why and how does any particular plant produce just those substances which are necessary for the development of its own peculiar forms of leaf, flower and fruit; and how is the appropriate distribution of these substances in the plant effected at just the right time and in proportionate quantity? We must admit that Science has not yet lifted the veil which enshrouds this last mystery of metamorphosis.

I have now concluded my task. I have laid before you to the best of my ability the history of the origin and growth of the doctrine of metamorphosis. With regard to

¹ Blumenbach's *Nisus formativus* (Ueb. den Bildungstrieb und das Zeugungsgeschäfte, 1781): see Vöchting, Ueb. Organbildung im Pflanzenreich, i., 1878.

² Von Sachs, "Stoff und Form der Pflanzenorgane," *Arb. d. bot. Inst. in Würzburg*, ii., 1882.

³ Goebel, *Vergleichende Entwicklungsgeschichte*, p. 113.

the present position of the doctrine, we have seen that the result of the vast amount of research which has been carried on since it was first promulgated, has but served to establish it upon a broader and sounder basis. Moreover it has been pointed out that the concept of a material metamorphosis—that is of a genetic relation between the different forms of the same member—is the one which has the weight of evidence in its favour ; though opinions are still divided, in the case of the leaf, as to whether the primitive form was a sporophyll or a foliage-leaf. In using the expression “material metamorphosis,” it is not implied that every metamorphosed organ once actually possessed the form of its primitive member, assuming it as a necessary phase in its development ; on the contrary, the idea involved is that, whilst all the embryonic organ-rudiments are potentially capable of developing into their appropriate primitive form, many of them do actually develop, under normal conditions, more or less directly into secondary, metamorphosed forms. It will also have been gathered that metamorphosis has a deep physiological significance, inasmuch as it is by this means that the few primitive members of the plant become multifariously adapted as organs for the performance of a great variety of functions ; and herein the leaf is prominent as the most plastic member of the body. In fact, a higher degree of metamorphosis is the external expression of a more complete division of physiological labour.

But if the store of knowledge on the subject has amazingly increased since the time of Wolff and of Goethe, it is still far from complete. This is a confession which has always to be made when stock is taken of what is known on any scientific subject ; for in no branch of Science is finality even in sight, and it is doubtless this that lends charm to the pursuit. In this subject we have still to discover the mysterious law of which Goethe speaks in the lines :—

Alle Gestalten sind ähnlich und keine gleicht der andern ;
Und so deutet das Chor auf ein geheimes Gesetz,
Auf ein heiliges Räthsel.

S. H. VINES.

THE BACILLUS OF PLAGUE.

AMONG the services rendered by bacteriology to medicine, not the least is the discovery of the micro-organisms which are the cause of two diseases that have lately reappeared in epidemic form. Both influenza and the plague, diseases well known to our forefathers, have been proved to be of bacterial origin, and as the result a mass of conflicting and contradictory evidence on the causation of these diseases has been cleared away.

Although the recent spread of the plague in China and India has drawn the attention of most civilised governments to the question of how the possible introduction of this disease into Europe may be prevented, it is worth noting that during the last forty years from 1855-95 the plague has appeared more than sixty times as large or small epidemics. Mahé (1) has pointed out that most of these outbreaks have occurred in Southern China, but on eighteen separate occasions it has been seen in Persia, twenty-six times in Turkey and five in Turkestan. One epidemic is recorded in South-east Russia, while in India, especially in the districts of Kumaon and Garwhal, the plague has appeared at least six or seven times. A study of the outbreaks in Marseilles in 1720, in Moscow in 1770, in Cairo and Smyrna in 1834, together with the one so well described by Russell in Aleppo during 1760-63, was until four years ago the chief source of our knowledge of the causation, symptoms and progress of the plague; and although no material advance has been made in the clinical history of the disease, it is interesting to recall some ideas which were current as to its causation. "During the continuance of the epidemic effect," writes an author fifty years ago (2), "a principle is given off from the body which, if very concentrated and pent up in confined and unwholesome situations, may generate the disease, so that, though not originally contagious, it may in this way by accumulation of animal miasms be contagious; and when the disease is communi-

cated from person to person it is by the inhaling the pestiferous breath or exhalations which emanate from the body of the patient. The communication of plague by inoculation with the matter of a bubo or other morbid product has been by no means proved; on the contrary, there is every reason to believe that the disease cannot be produced by these means." Vague conceptions such as these are now fortunately only rarely met with in medical literature since inquiries into the etiology of disease are at the present time conducted by methods, and upon lines identical with those that have advanced natural and physical science.

On the 14th of June, 1894, Kitasato of Tokio discovered the specific microbe of plague during an outbreak in Hong Kong (3). Independently Yersin (4) made the same discovery a few weeks later in the same year; and to this micro-organism, the pest-bacillus or *Bacillus pestis bubonicæ* the name of *Coccobacillus pestis* has been given by Metchnikoff (5). The majority of bacteriologists regard the bacillus of Kitasato as identical with that of Yersin, but according to Ogata (6), whose researches were made in Formosa during 1896, this is by no means certain, and he adduces among other evidence a statement to that effect made by Kitasato himself. Although it is only to be expected that two original researches on the same subject should exhibit certain differences, which undoubtedly did exist as to the mode of growth, staining, behaviour and motile power of the bacillus, the discrepancies between the two observers are not sufficient to justify the attitude taken by Ogata, Aoyama, Okada and other Japanese observers, which aims at discrediting Kitasato's work. His researches, however, are confined to the actual discovery of the bacillus and the proof of its pathogenic nature.

An excellent account by Kruse in the third edition of *Die Mikro-organismen*, by C. Flügge, contains all that was known of the pest-bacillus in 1895, but since that date numerous papers on this subject have appeared, and the reports of the Scientific Commissions sent to India by most European Governments for the purpose of studying the outbreak of plague in that country have been published,

so that there now exists a large amount of information, not only on the etiology, but also on the modes of communication, means of spread, pathology, and treatment of this disease. In the communication made by Metchnikoff at the International Medical Congress in Moscow, August, 1897, is found an admirable exposition of the nature of the plague (7).

As described by Yersin the specific microbe of plague is a very short bacillus with rounded ends and apparent vacuolation in the centre. The organism, which is not endowed with movement, is often surrounded by a capsule, and Zettnow (8) has demonstrated this by Löffler's method for staining the flagella of bacteria. He considers that the capsule represents the plasma of the bacterial cell, while the microbe itself is a nucleus. By van Ermengem's method Gordon (9) has succeeded in staining a spiral flagellum about twice the length of the bacillus, which is attached to this only at one extremity; occasionally a second flagellum can be seen close to the other. The specimens showing this were obtained from agar cultures of twenty hours' growth at a temperature of 37° C. which had been made from the blood of an inoculated animal. If examined in hanging drops this observer states that the pest-bacilli are to a slight extent motile, a view which is shared by no other worker. With the usual aniline dyes the bacillus stains well, especially at the ends rather than the centre, so that an appearance is presented like the bacillus of chicken-cholera. By the method of Gram it does not stain but becomes tinted if a contrast colour is used. Two characters of the bacillus render it difficult to recognise microscopically, a marked tendency to the production of polymorphic and involution forms, but both these characteristics have been utilised as aids to diagnosis. Klein (10) has pointed out that gelatine cultures twenty-four to twenty-eight hours old show atypical thread-like colonies which resemble those of *Proteus vulgaris*, and the presence of these colonies which have also been noted by other observers in older cultures he considers may be regarded as characteristic of the pest-bacillus. Hankin and Leumann (11) describe well-marked

and even exaggerated involution forms which can under favourable conditions be seen in twenty-four hours if cultures are made on agar which contains 2.5-3.5 per cent of salt. If kept at a temperature of 37° C. extraordinary involution forms are met with. Large spheres and pear-shaped bodies which often have rod-like extensions are seen, but at later periods of growth these very abnormal forms are not frequently met with. For the success of this method as a test for the bacillus it is necessary to first cultivate the microbe on agar and then transfer it to the salted medium. Haffkine had previously recommended the use of agar having a well-marked alkaline reaction for the rapid production of involution forms, but since it is not possible to keep this medium long at the required degree of dryness, or to obtain such certain and quick results, it is not to be preferred to a salt-agar medium.

The pest-bacillus is easily cultivated outside the body, and indeed grows well on all varieties of media. One of the best of these is the one originally recommended by Yersin, whose statement is confirmed by Wilm (12). It consists of 2 per cent. alkaline peptone solution with the addition of 1-2 per cent. of gelatine. Both on this and in ordinary bouillon made from horse-flesh a culture is obtained which much resembles that of streptococcus pyogenes; flocculent deposits forming at the bottom and sides of the tube, while the rest of the liquid remains clear. At times this liquid becomes diffusely turbid, and these two types of growth in bouillon are produced, according to Abel (13), who worked with a culture derived from Kitasato's original stock and one obtained from a fatal case of plague in London during October, 1896, by the manner in which the medium is inoculated. Neither on Löffler's blood-serum on coagulated ascitic fluid, on gelatine, which is never liquefied by the growth, on glycerine-agar, nor on agar-agar, does the bacillus thrive so well as in bouillon. On the last of the solid media a maximum rate of growth is reached in twenty-four to forty-eight hours at the body temperature, and isolated white transparent colonies with iridescent edges are formed. These colonies differ in size and rate of growth,

the larger ones according to Yersin, show a diminution in virulence similar to that which is seen when examining a growth obtained after successive cultivation. On potatoes the bacillus grows feebly at a temperature of 37° C., and in milk a slight increase can be seen, but no curdling occurs (Abel). No growth at all was seen in milk by Kolle (14), though Wilm has described one accompanied by coagulation. The plague-bacillus grows under both aerobic and anaerobic conditions, develops no gas in media containing sugar, and no indol reaction can be obtained from bouillon cultures. An acid is developed in litmus-bouillon in twenty-four hours, and this in no way can hinder growth, since a degree of acidity such that 10.5 c.c. of decinormal soda was required to neutralise 100 c.c. of an acid bouillon that still remained a good culture-medium. A study of the respiration of the bacillus has been made by Hesse (15). Both the assumption of oxygen and evolution of carbon dioxide show variations, the maximum of which corresponds to the period of most energetic growth and is reached on the fourth or fifth day. A larger volume of oxygen is taken up than the carbon-dioxide liberated, the two processes, like the respiration of all protoplasm, being quite independent of each other. Anaerobic cultures in an atmosphere of hydrogen did not succeed at all on agar, and only to a limited extent on gelatine, showed that a diminished amount of CO_2 was evolved.

When compared with most other pathogenic micro-organisms the bacillus of plague yields rather scanty cultures which require some care for preservation, and it is easily killed by weak solutions of disinfectants like carbolic acid, lyssol, mercury perchloride and milk of lime. The last of these as has been shown by Kitasato, Wilm and others to effectively kill a culture in ten minutes to three hours, according as the added lime varies from .5-5 per cent. in a bouillon culture. Early experiments made by Kitasato have proved that the Pest-bacillus, like many other non-sporing bacteria, is easily killed by drying. The microbes or pieces of tissue from plague-infected animals become non-infective by the removal of water, and the

rapidity with which this is done either in an exsiccator or by exposure to varying temperatures dermines the result. At 29° - 31° C. the bacilli are killed in four and a half days, and in three hours if dried over sulphuric acid. When the drying process is slow and carried out in the dark at 16° - 20° C. living bacilli are found on the thirtieth day. If in Hong Kong at a temperature of 28° - 31° C. four and a half days is needed, then for our climate about ten days would probably suffice to kill the microbe. Direct sunlight and dry heat destroy cultures of the bacillus with certainty, but steam at 50° C. for one hour renders all cultures harmless, and the following table by R. Abel show this to be a most effective means of destruction :—

At 100° C. all bacilli dead at the end of 1 min.				
„	80°	„	„	5 „
„	70°	„	„	10 „
„	60°	„	„	20 „
„	50°	„ some alive, some dead at the end of 30 min.		
„	50°	„ all dead	„ „	60 „

The pest-bacillus is pathogenic for man and a great number of animals, especially mammals, though the symptoms, progress and termination of the disease are not quite similar in both cases. Gaffky, R. Pfeiffer, Dieudonné and Sticker (25), members of the German Plague Commission recognise three different clinical varieties of the plague. In the true bubonic plague there is a sudden onset of high fever accompanied by characteristic swellings of the superficial lymphatic glands in the groin and the axilla, which form the buboes of typical plague and contain almost pure cultures of specific bacilli with or without the association of pyogenic streptococci. Pest-septicæmia is another form of the disease where the spleen is always enlarged and signs of general sepsis with hæmorrhages into the stomach and bowels are found on autopsy. The third form of plague is Pest-pneumonia in which numerous specific microbes can be recognised in the sputum either alone or associated with diplococcus lanceolatus and pyogenic streptococci. Among human beings the mortality is as high as 80 per cent. After an incubation period which is stated by Lowson (16) to be

about three to six or even nine days, with the spread of the bacilli throughout the body which on *post mortem* are found distributed in blood, lymph, the spleen, marrow, brain, liver, lungs and lymphatic glands, death generally occurs on the third to the seventh day. An excellent clinical history of the plague in Formosa, 1896, together with the macroscopic and microscopic morbid anatomy of cases is given by Yamagiwa (17). A study of the acute hæmorrhagic inflammations in the organs of animals killed by the plague has been made by Honl (18), who speaks of a definite purpura pestica, which is a primary septicæmic condition fatal in two days, and a purpura streptococcica which is a secondary hæmorrhagic infection seen in later stages of the disease. In the spleen and lymphatic glands, by means of sections treated with hæmatoxylin, counter-staining with concentrated aqueous solutions of methylene-blue and subsequent impregnation with tannin, numerous accumulations of zooglœa masses are to be found, in which the separate bacilli are seen separated from each other by a homogeneous envelope. From histological evidence V. Babes and C. Livadite (19) consider the plague-bacillus as a representative type of the group of bacteria that cause specific hæmorrhagic infection in man and animals. They describe and figure distinct hæmorrhagic zones in dilated capillary districts, in which masses of bacteria are seen that have broken through the walls of the vessels and so established innumerable hæmorrhages. They think that is not necessary to regard these as being produced by any bacterial toxin which injures the vessels, or to believe that certain nerve centres are implicated and in this way produce vascular changes, but that the characteristic hæmorrhages are directly produced by the pest-bacilli. It may be pointed out, however, that Charrin, Unna and other observers deny the existence of specific hæmorrhagic bacteria, and, while not disputing the facts, consider that hæmorrhages are dependent on a peculiar disposition found in certain men and animals, and due to such a variety of causes that hæmorrhagic bacteria, in the sense that Babes speaks of these, cannot be ranked as specific micro-organisms.

It is an old observation that the plague which is at first a disease of rats and mice soon becomes a disease of man, and all recent authors confirm this fact. That these animals are really plague-stricken admits of no doubt. In Formosa plague is known as rat-pest, and in Canton one man alone collected 22,000 dead rats during the outbreak in 1894. Animals, as laboratory experiments prove, may succumb either by ingestion of pieces of infective organs or by inoculation at the surface of the body. When passed through a series of animals the microbe, which on cultivation outside the body is easily attenuated, acquires increased virulence, in this respect resembling other bacteria such as those of anthrax and chicken-cholera. H. F. Nuttall (20) has shown that outbreaks of plague in various countries have been described as preceded by or associated with the death of various animals besides rats and mice. Not only these, but also pigs, cats, dogs, snakes, horses, buffaloes, goats, fowls and other birds have been stated to be affected. Experiments conducted by Nuttall (20), Yersin, Wilm, Galeotti and Malenchini (21), Lowson and Devell (22) at the same time restrict and add to our knowledge of the susceptibility of different animals, which after subcutaneous inoculation, die in a few days with symptoms of septicæmic poisoning accompanied by a strong reaction at the seat of the injection. Bubonic swellings of the glands may or may not be present. According to Nuttall's tables the following animals are susceptible: rats, white mice, house mice, shrew mice, guinea-pigs, rabbits, pigs, horses, monkeys, cats, fowls and lizards; while pigeons, dogs, oxen, the tortoise and frogs are immune. One species of the latter (*Rana temporaria*) is stated by Devell (22) to die of plague if bacterial cultures or pieces of infected organs are introduced into the lymph sac of the animal. A microbe of virulence sufficient to kill mice in two to two and a half days requires thirteen to seventeen days for a frog, but by passage through this animal the virulence of the bacillus is augmented and the time so shortened that death follows inoculation on the fifth day. Many animals which under normal circumstances are immune to bacterial in-

jection, become susceptible when they are starved or fatigued, and pigeons if food is withheld lose their natural resistance to plague (28). Although the pest-bacillus is pathogenic for a large number of animals it is by no means proved that during an epidemic they actually convey the disease, though, of course, this is conceivable. Hankin (23) has shown that there is no necessary connection between infection of animals and outbreaks among men. In many localities near Bombay after the disease had been introduced by human beings, it ran its course without a single rat being affected, and in the neighbourhood of Hurdwar a genuine outbreak occurred among rats and terminated without spreading to the community.

In the bodies of plague-stricken individuals specific microbes can be easily recognised in the glandular swellings, in pus and the blood. Their presence in the latter affords a certain means of diagnosis in doubtful cases, but since comparatively few pest-bacilli are met with in blood a large number of specimens must be made and care taken not to mistake those which are seen for streptococci which are not infrequently present. By blood examination Lowson found the pest-bacillus in 80 per cent. of cases and Wilm in 77 per cent. The latter observer considers that a more certain diagnosis may be obtained by cultivating the suspected blood in bouillon, and has also recommended an examination of the urine which always contains plague-bacilli, not only during the disease but for at least a week after recovery. Two members of the Russian Plague Commission (24) have also shown that the blood serum of patients convalescent from plague has the power of causing an agglutination of the bacilli in a hanging drop or bouillon culture, similar to that originally observed by Chantermesse and Widal when the blood of patients suffering from typhoid-fever was added to cultures of bacillus typhosus or the serum of cholera to cholera microbes. This phenomenon of agglutination in cases of plague is not seen during the first week, but appears by the seventh day, persists until the fourth week and then finally is lost. The members of the German Plague Commission (25) have also noted this re-

action which enables the pest-bacilli to be recognised with certainty.

Infection by the bacillus in the majority of cases is at the surface of the body, minute wounds and abrasions of the skin affording a means of entrance. More rarely the mucous membranes of the intestine are primarily affected, and though Wilm believes the majority of cases in Hong Kong were infected in this way, the reports of the various commissions in Bombay state that this, though it may occur, is rarely seen. Experimentally animals may be infected at the intestinal or respiratory surfaces and pest-bacilli are frequently found in sputa and always in the excreta. Lesions in the mouth or tonsils are also sites where the bacilli may obtain an entrance into the body, but apparently these never primarily infect by the mucous membrane of the stomach (24). The cells lining the alimentary canal undoubtedly oppose the entrance of bacteria. Most careful observations recently made by Neisser (26) show that chyle and blood are absolutely free from germs even after the liberal ingestion of pathogenic bacteria. The mesenteric glands remain normal, and it is only when the mucous membrane is diseased that bacteria can leave the cavity of the intestine, and even then these are not absorbed, but grow through the wall of the bowel in a manner comparable to the growth which is known to take place through the substance of a filter. Wilm and Abel have both examined the behaviour of pest-bacilli in water, since it is conceivable that this may be a vehicle for transmission of the specific germs. The former observer found bacilli lived for twenty days in distilled water, in spring water sixteen, and in sea water six days, 200 c.c. of water being mixed with .5 c.c. of an agar culture. Abel's researches also show that the specific microbes of plague can live for weeks in water, so that by washing in this there will be a possible risk of infection. Gaffky and his colleagues however have shown that in absolutely sterile water the bacilli live only three days and in ordinary tap-water for one day, so that plague cannot be considered to be a water-borne disease.

The part played by insects in the spread of plague has

been studied by Nuttall (20). It had been already noticed by Yersin (4) that flies died of plague, and that an infected insect could establish the disease in a guinea-pig. Many flies, however, die of water, and the laboratory experiments by Yersin have been greatly extended by Nuttall's researches, who has worked out this subject with the greatest care, making use of cultures from Kolle and A. Macfadyean of London, which were sufficiently virulent to kill mice in thirty-six to forty-eight hours. To any one familiar with the swarms of flies in the East, it would appear very probable that if these insects can be infected, which undoubtedly is the case, they might be an efficient means in spreading the disease. In all experiments to decide this question control experiments were devised. Flies fed with infected bouillon died much earlier than those in the control apparatus, and their bodies contained pest-bacilli which were pathogenic for mice. Infected flies which had not died contained virulent bacilli for forty-eight hours and longer, after they were removed from the apparatus. Similar experiments were made with bugs, but in these insects the bacilli gradually die. In the case of mosquitoes and fleas no experimental evidence exists, though it is a well-established fact that the former can infect man with filariæ, and virulent pest-bacilli have been found in fleas taken from plague-stricken rats (25). Hankin has observed that ants rapidly eat up rats dead of plague, and their excreta contains virulent bacilli. Ants, however, do not die of plague, nor do they retain the infection for any length of time (27). From a practical point of view, though the actual proof in any epidemic has not yet been given, it cannot be denied that flies at any rate may play a part in spreading the plague, and at any rate may contaminate food or water by their excreta.

Like other pathogenic bacteria the bacillus of plague produces toxins, which accumulate in the micro-organisms and are discharged by these into the surrounding medium. The fever of plague is directly due to these products. Lustig and Galeotti (29) have obtained various products by extracting cultures of the microbe for twelve to twenty-four

hours at a temperature of 10° - 12° C. with .75 per cent. potassium hydrate. This treatment yields a viscid opalescent fluid which is filtered through paper, and the precipitate obtained from the filtrate by precipitation with weak acetic acid is collected, washed, dried and dissolved in sodium carbonate. Prepared in this way the solution probably contains among other bodies a nucleo-proteid, and is found to be toxic for rats and mice. The same substance if injected in non-lethal doses, either subcutaneously or into the peritoneal cavity, confers immunity to infection by virulent pest-bacilli, and this condition lasts for about four weeks, and the serum of the blood of these animals during this time possesses both an immunising and protective power. In the method devised by Roux the pest-bacillus is rendered exceedingly virulent by introducing microbes enclosed in small collodion bags into the peritoneal cavity of the rabbit. Within the sacs a free development takes place, and the bacilli acquire a high degree of virulence. If these are now grown upon gelatine bouillon the toxic products of the bacteria can be separated by filtration through a Pasteur-Chamberland tube. A still more active toxin is obtained if the bacilli are allowed to die in the culture fluid, which then extracts the toxin. The clear fluid rich in toxin is precipitated by ammonium sulphate, and the deposit collected and dried. The activity of this powder is such that .25 milligrammes will kill mice in a few hours. There is nothing of special interest connected with the toxin of the plague-bacillus. It is obviously a mixture of several substances, some of which are of a proteid character. The chemistry of this, as of other toxins, is a matter of conjecture, and this is recognised only by its effects, for there is no doubt that by small repeated injections a state of immunity can be conferred on animals otherwise susceptible to infection with living plague-bacilli.

The prophylactic and curative power of the serum of animals that have been experimentally brought to a high grade of acquired immunity by the introduction of toxins, dead bacilli or attenuated cultures into the system has been extensively studied by Yersin (31), Haffkine (32),

Calmette and Borrel (33), Galeotti and Malenchini (34), Wladimiroff (35) and others, and the members of the several Plague Commissions in Bombay have furnished long reports on the value of the serum treatment in plague as a preventive and therapeutic measure. In Haffkine's method, which is similar in principle to that practised by him as a protection against cholera and typhoid, a fluid preparation obtained from dead cultures of pest-bacilli is employed; 2.5 c.c. is injected into the forearm of an adult; fever of slight amount accompanied by pain and swelling at the seat of injection are the chief phenomena of the reaction. This treatment affords considerable but not absolute protection, and its beneficial effect tends to disappear. Yersin, Calmette and Borrel consider filtrates of cultures which contain toxin are useless for protective purposes. By employing the serum of horses that have been immunised by subcutaneous injection of cultures of the pest-bacillus warmed to 58° C., mice and other animals can both be protected from and cured of plague. That serum so prepared has specific anti-toxic power appears to be proved, since the serum of normal horses, the anti-diphtheritic serum of Roux, the antitetanic serum of Roux and Vaillard, the antivenin of Calmette and the antistreptococcic serum of Marmorek, were all found to be useless as a means of protection against an infection by virulent plague-bacilli. An analysis of cases treated by Yersin's serum shows that while the mortality in hospitals is 80 per cent. of those attacked, this number is reduced to 49 per cent. By experiments made on monkeys (*Semnopithecus entellus* the grey, and *Macacus radiatus* the brown ape, the latter of which is remarkably susceptible to plague) Galeotti and Malenchini have established the therapeutic and protective value of the serum of horses that have been rendered immune by the repeated injection of small amounts of toxin.

With the development of bacteriology not only have the specific forms that determine disease been isolated, but the progress of this science has shown all civilised countries the way to guard against the introduction of such a scourge as plague, which in former centuries half depopulated the large

cities of Europe. The therapeutic use of antitoxic sera, which is admitted by all who are competent to form an opinion to be of the greatest use in tetanus, diphtheria and snake-bite, is the direct outcome of bacteriological work, and the services rendered by this science to practical medicine in such a disease as the plague can hardly be over-estimated.

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G. A. BUCKMASTER.

SECRETION AND ABSORPTION OF GAS IN THE SWIMMING-BLADDER AND LUNGS.

PART I.—SWIMMING-BLADDER.

IN the present paper I propose to give a short account of what has been ascertained with regard to the conditions which determine separation and absorption of gas by the swimming- or air-bladder. The subject is one of considerable interest, since it seems to lead up very directly to certain questions of fundamental importance in biology.

The air-bladder, as is well known, is an elongated sac containing gas, and usually lies dorsal to the alimentary canal, of which it is an out-growth. It is present in most, but not in all, fishes. The duct communicating with the alimentary canal is in some species permeable, and in others completely closed.

It has been generally assumed for long that the chief function of the air-bladder is to enable the animal to increase or diminish its specific gravity, and thus alter the depth at which it swims. Borelli in his famous book, *De Motu Animalium*, says that when it wishes to go down, the fish, by means of its muscles, compresses the air-bladder, and thus diminishes the volume and correspondingly increases the specific gravity of its body. When the fish wishes to rise it relaxes its muscles, and thus diminishes the specific gravity.

Another theory which deserves mention is that of Delaroche,¹ who supposed that the muscles which compress the swimming-bladder are in a state of tonic contraction, regulated according to the depth at which the animal wishes to remain. When the animal rises or goes down the contraction becomes more, or less, vigorous, so that the specific gravity is kept constant. The function of the air-bladder is thus to enable the animal to remain at any desired depth in the water.

The whole subject of the functions of the air-bladder was carefully investigated about twenty years ago by

¹ *Annales du Muséum d'histoire naturelle*, 1809.

Moreau,¹ whose work, however, does not seem by any means so well known as it ought to be. He proved that the current ideas about the mode of action of the air-bladder were entirely erroneous, and that a fish in reality makes no use of its muscles in regulating the volume of its air-bladder. When the fish descends, the air in the bladder is compressed and the specific gravity of the body increases, so that it naturally tends to sink farther and farther, and if it got too far would sink to the bottom. Conversely, if the fish ascends its air-bladder expands, and if it goes too far it is helplessly carried to the surface, unless it possesses a permeable air-duct or the air-bladder bursts outwards. Thus

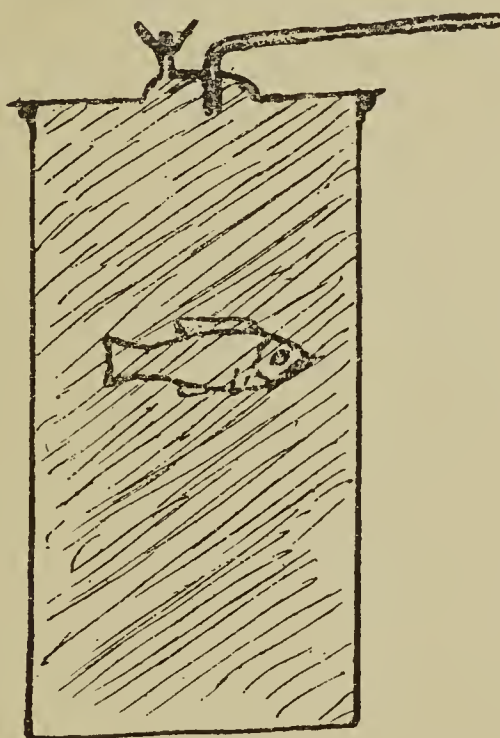


FIG. 1.

the air-bladder of a fish tends to make it behave exactly like the toy known as the "Cartesian Diver". From this point of view it would seem as if an air-bladder must be a source of great inconvenience to a fish, exposing it to a constant danger of being carried helplessly to the surface or to the bottom. Its position at the best is evidently one of unstable equilibrium, which the smallest movement upwards or downwards could disturb.

Moreau demonstrated the facts just mentioned by means of several experiments, one or two of which may be described. He placed the fish in a large and tall glass vessel completely filled with water (Fig. 1). Into this vessel

¹ *Mémoires de Physiologie*, Paris, 1877.

there was fixed at the top a glass tube, with a horizontal arm. Any change in the volume of the fish was of course indicated by a movement in the liquid in this tube. He found that as the fish swam upwards its volume expanded, as shown by the movement of the water in the horizontal tube, and conversely as it swam downwards. The movements of the water in the tube always corresponded exactly to the height of the fish, just as if the latter had been a "Cartesian Diver," pulled up and down by a thread. Similar results were obtained when, by means of slightly different experimental arrangements, the pressure

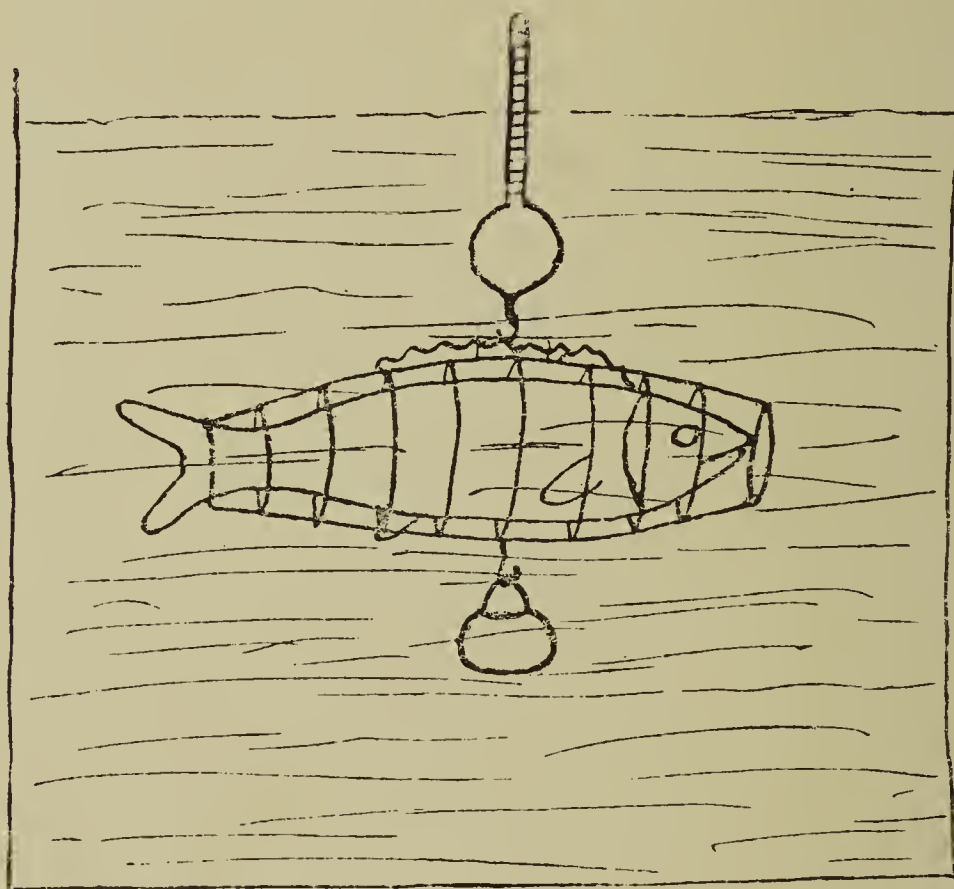


FIG. 2.

inside the vessel was artificially raised or lowered. Thus the fish made no use of its muscles to compress its air-bladder. Compression of the air-bladder, when it did occur, seemed to be merely accidental and momentary, as when the animal made a sudden and violent movement. Even with a species possessing a specially muscular swimming-bladder the result was precisely the same. If the fish was confined in a closely fitting wire cage, so that it was unable to move its tail or fins (Fig. 2), or if it was anæsthetised with ether, it behaved, when subjected to differences of pressure, exactly like a "Cartesian Diver".

Moreau's further experiments showed that the air-bladder may in reality so act as to assist the animal in balancing itself at any required depth, but that the action is an exceedingly slow one, and not dependent in any way on the muscles. He took a fish which had been living at the surface and determined its specific gravity by means of the arrangement shown in Fig. 2. He then sank the fish in a cage to a considerably greater depth, and after about two days pulled it up again, and redetermined its specific gravity at the same pressure as before. He always found that the fish was now much lighter. It had gradually secreted air into its air-bladder while at the deep level, so that at last its specific gravity corresponded with that of the water. On coming up again the extra air of course caused it to swell, so that it was much lighter when brought back to its old level.

When the air-bladder was artificially emptied, either by puncture with a trocar, or (in the case of fish with a patent duct) by producing a vacuum in the vessel containing the animal, the immediate result was that the fish sank helplessly to the bottom. After a few hours, however, it was able to swim about again as usual, gas having gradually been secreted into the air-bladder. This experiment may very conveniently be performed on a goldfish. All that is necessary is to place the animal in a large-sized bottle provided with a perforated rubber cork, through which passes a tube connected with a filter-pump. As soon as sufficient air has escaped from the swimming-bladder the tube is removed. The fish at first remains helplessly at the bottom of the bottle, but gradually recovers, and next morning will be swimming about again as usual. Another experiment described by Moreau is to take two fishes, and attach a float to one and a sinker to the other. At first the fish with the float is unable to leave the surface, while that with the sinker is unable to leave the bottom. After some hours, however, the two fishes will be swimming about together as usual, the fish with the float having absorbed some of the gas in its air-bladder, while the fish with the sinker has secreted gas. This experiment seems to indicate that the

secretion and absorption of gas is in some sense under voluntary control. On increasing or diminishing the pressure within a large vessel of water in which a fish is swimming, it is easy to see that the animal by swimming downwards or upwards endeavours to counteract the tendency of the expansion or compression of the air in the swimming-bladder to carry it in the opposite direction. The fish is evidently made very uncomfortable by alterations in the volume of its air-bladder, and the discomfort causes it to assume the depth at which the volume of the air-bladder is normal. An animal swimming in the sea will thus tend to be kept at about one depth. Permanent alteration of depth will at any rate only be brought about gradually. The air-bladder thus seems to be a contrivance of such a nature as to prevent a fish from suddenly altering, except to a very limited extent, the depth at which it lives. Presumably it is of advantage to the animal to remain at about one level; on the other hand, gradual changes of depth will be compensated for by secretion or absorption of gas, so that if the change is only made gradually enough the animal will be able to live equally comfortably at the most varying depths. Moreau remarks that flat fish have no air-bladders, and this is what might be expected from the fact that they rest on the bottom on their sides.

I now come to the facts with regard to the air-bladder which are of the chief interest to physiologists. Early in the present century it was found by Biot¹ and other observers that the gas in the swimming-bladder is not air, but a variable mixture of oxygen and nitrogen, together with about 1 or 2 per cent. of carbonic acid. The most remarkable fact discovered about this mixture, however, was that it frequently consisted almost entirely of oxygen, especially in deep-sea fishes.

The following table gives the results of a number of analyses by Biot:—

¹ *Mémoires de la Société d'Arcueil*, 1807.

BIOT'S ANALYSES OF GAS FROM THE AIR-BLADDER.

Name of Fish.	Depth at which it was caught.	Percentage of oxygen in the air-bladder.
Mugil cephalus - -	Close to surface	Traces
Mugil cephalus - -	" "	"
Sparus annularis ♂ -	" "	9
Sparus annularis ♂ -	" "	8
Sparus sargus ♂ -	43 feet	9
Sparus sargus ♂ -	" "	20
Holocentrus marinus -	" "	12
Sparus melanurus -	Close to surface	20
Labrus turdus - -	12 feet	16, 24 and 28
Labrus turdus - -	43 "	24
Sciaena nigra - -	" "	27 and 25
Sparus dentex - -	120 "	40
Sparus argenteus -	390 "	50
Holocentrus gigas -	Great depth ¹	69
Gadus merluccius -	" "	79
Trygla lyra - -	" "	87

By Johannes Müller and other physiologists this gas was spoken of as a secretion, but ideas about secretion were then still in their infancy. Müller, it is true, possessed what appears to be the right idea, that all true secretion is, in the most intimate sense, a "vital process," and that therefore the problem of the cause of secretion directly involves the whole question of the cause of life itself. This idea of Müller's was, however, for the time lost sight of in the course of the movement towards physico-chemical speculation which overwhelmed physiology about the middle of the present century.

Biot draws attention to the fact (evident from the table of his analyses) that the greater the depth at which a fish is caught the higher the percentage of oxygen in its air-bladder. Moreau confirms this; and his experiments throw further light on the subject. He found that if the air-bladder was artificially emptied, either by puncture or by reduction of atmospheric pressure, the gas which is afterwards gradually secreted into the bladder is richer in oxygen than before, and usually far richer in

¹ Often caught at 3000 feet.

oxygen than air. It would seem therefore that pure, or nearly pure, oxygen is secreted into the bladder. A similar result is obtained by sinking a surface fish into deep water in a cage. As much as 90 per cent. of oxygen may be present in the gas from the bladder.

It is generally assumed that the passage of gases throughout the body, and between the body and its environment, is due simply to diffusion. This theory is probably perfectly satisfactory for many cases, but does not account for the facts connected with the swimming-bladder. Supposing the bladder to have been originally filled with air, which had passed in, it might be, through the duct when the fish was young, what would happen to this air on the diffusion theory? In the first place its oxygen percentage would never increase. The amounts of oxygen and nitrogen dissolved in sea water depend on the tensions or partial pressures of each of these gases in the atmosphere, and on their co-efficients of solubility. Sea water is known to be saturated with nitrogen, and more or less saturated with oxygen at the pressures which these gases respectively exercise in air. Supposing some of this gas to come in contact with an atmosphere of gas in which the partial pressure of nitrogen or oxygen was less than in air, then nitrogen or oxygen would be given off from the water until a state of equilibrium was again established. Conversely, if the partial pressure of nitrogen or oxygen in the gas was greater than in air the water would take up these gases. The air and the water have the same gas-tensions when the equilibrium is established. Two liquids, such as sea water and the blood passing through the gills, will by simple diffusion tend to assume the same oxygen, nitrogen and carbonic acid tensions. On the diffusion theory the blood of course cannot have a higher oxygen tension than the air, hence it cannot give up oxygen to the air-bladder if the latter already contains oxygen at atmospheric tension. Actually, however, the oxygen tension of blood passing through a tissue such as the wall of the air-bladder will be less than that of air, owing to the using up of oxygen for respiratory purposes. Hence the blood will absorb oxygen

from air in the air-bladder. The consequence of this will be that the nitrogen percentage in the air-bladder will be increased, and as the total pressure within the air-bladder is at any rate not less than that of the atmosphere, the partial pressure or tension of the nitrogen in the air-bladder will be greater than that of air, and therefore greater than the nitrogen tension of the blood. The nitrogen of the air-bladder will therefore follow the oxygen, and finally, there will be no gas of any sort left. If the fish is much below the surface the process of absorption of gas from the air-bladder will be far more rapid since the partial pressures of its gases will then be far above the partial pressures of the corresponding gases in the atmosphere or in the water. Every thirty feet of depth will increase by about an atmosphere the pressure of the gases in the air-bladder. Thus on the diffusion theory all the gas ought to disappear from the air-bladder, whereas actually the gas in it remains unabsorbed, or may even increase.

When gas is from any cause introduced into any closed space in the body, such as the pleural cavity or the blood-vessels, absorption does actually occur, and this absorption is rendered intelligible by the explanation just given on the diffusion theory. The same explanation also applies to the well-known fact that when the Eustachian tube is blocked by catarrh a partial vacuum tends to be produced in the middle ear, causing deafness from collapse of the membrana tympani, etc.

When the fish goes far below the surface of the water the pressure in the air-bladder of course increases enormously. Hence the partial pressure of one or all of the gases in the air-bladder rises high above the partial pressure of the same gases in the water, particularly in the case of oxygen, when the bladder contains a high percentage of the latter gas. Consequently, on the diffusion theory, these gases ought to be rapidly absorbed. At the great depths at which some fish are caught, the partial pressures of the gases in the air-bladder are enormous. To take an example, in the case of a fish (*Synphobranchus pinnatus*) caught at a depth of 4500 feet, the gas from the air-bladder

was found by Schloesing and Richard¹ to contain 85 per cent. of oxygen and 12 per cent. of nitrogen and argon. The pressure at this depth is about 150 atmospheres, hence the tension of the oxygen in the bladder was about 127 atmospheres, and of the nitrogen about 18 atmospheres, as compared with tensions of a fifth and four-fifths of an atmosphere respectively in the sea-water. At such a pressure complete absorption of the gas ought, on the diffusion theory, to occur with the utmost rapidity, whereas nothing of the sort happens, and these fishes must be able to secrete oxygen and nitrogen against these enormous pressures. The fact is the more remarkable since oxygen at anything more than 4 or 5 atmospheres' pressure is exceedingly poisonous to both animals and plants. Yet the wall of the air-bladder is not susceptible to the poison. In this respect it may be compared with the wall of the stomach, which, so long as it is alive, resists the action of the exceedingly poisonous hydrochloric acid which it secretes. In *Dolium Galea* (or water snail) a digestive juice containing 4 per cent. of sulphuric acid is secreted, yet this corrosive liquid, which would instantly kill almost any other living tissue, does no harm to the glands which secrete it.

The gas secreted by the walls of the air-bladder seems usually to be a mixture of oxygen and nitrogen. The fact that with increasing depth the oxygen percentage as a rule increases would seem at first sight to suggest that oxygen alone is actually secreted, the nitrogen simply diffusing in until its partial pressure equals that of the nitrogen in the water. This hypothesis is however not tenable. The example quoted above shows that not only the oxygen tension, but also the nitrogen tension in the air-bladder, may far exceed that of the water. In some fishes, moreover, the gas contained in the air-bladder may be pure, or almost pure nitrogen. Thus in *Coregonus Acronius*, a freshwater fish living at a depth of 200 to 250 feet in the Lake

¹ *Cómptes Rendus*, vol. cxxii. (1896), p. 615. The writers suggest that the oxygen found by them may have been given off from the hæmoglobin of the blood while the animal was being drawn to the surface. Evidently, however, this cannot be the case.

of Geneva, Hüfner found that the gas consisted sometimes of perfectly pure nitrogen,¹ which must have had a tension of six or seven atmospheres. Schloesing and Richard found that the "nitrogen" from the air-bladder of *Synphobranchus pinnatus* contains about 1.94 per cent. of argon. The argon tension in the air-bladder at the depth at which the animal was caught must thus apparently have amounted to 35 per cent. of an atmosphere. Since the argon tension of sea-water is presumably only .93 per cent. of an atmosphere it would thus seem as if even argon may be secreted by the swimming-bladder.

Moreau noticed that section of the sympathetic nerve fibres going to the walls of the air-bladder seems to hasten the secretion of gas into the empty bladder, and more recently Bohr² has found that section of the vagus branch entirely stops it. Thus the secretion of gas, like secretion from the salivary or other glands, is under the control of the nervous system.

It has been known for long that in the walls of the air-bladder of many fishes there are present "retia miralulia," *i.e.*, bunches or discs of finely divided vessels. These structures are, however, beneath the lining epithelium, and it is difficult to see how they can have any very direct connection with the secretion of gas. The epithelium, however, is often differentiated into a more or less gland-like structure (the "epithelial body") which was described a few years ago by Coggi. Anything like full and complete investigations of these structures do not seem to have been made yet, although presumably the structures described by Coggi are real air glands.

As regards the process by which gas is secreted it is difficult to avoid the conclusion that the molecules of gas are liberated from some form of combination within the cells lining the air-bladder. It seems not unlikely that this process is continually going on, even while the actual amount of gas in the bladder is not increasing. Moreau

¹ *Archiv für Anatomie u. Physiologie*, 1892, p. 54. The argon was, of course, not determined.

² *Journ. of Physiol.*, vol. xv., p. 494.

has shown that when a fish is asphyxiated it quickly uses up the oxygen in its air-bladder. Evidently therefore the oxygen may pass outwards into the blood, and considering the physical properties of the air-bladder it is difficult to believe that diffusion outwards is not constantly occurring to a certain extent.

If the gas is liberated from combination within the cells, then we have in the animal kingdom a process which may be compared to the liberation of oxygen from the green parts of plants, and the fixation of free nitrogen by the parasitic organisms in Leguminosæ. Life is commonly regarded as being essentially an oxidation process, the activity of the green parts of plants in presence of light being looked upon as something exceptional, directly due in some way to the presence of a specialised pigment. The fact that free oxygen may be liberated even in the case of animals must tend to shake this belief, which in any case is rendered difficult by the fact that the only plausible theory to account for the presence of free oxygen in our atmosphere seems to be the presence of living organisms capable of liberating oxygen. Organisms capable of liberating oxygen must thus apparently have preceded in order of development those living on free oxygen. Physiologists in recent times have been apt to regard life too much from a physical and chemical, and too little from a biological, point of view, and it seems possible that just as morphological identity is concealed under the greatest diversities of physical form, so physiological identity is concealed under great diversities of physical and chemical process, and that comparative physiology, working by means of biological and not merely physical and chemical conceptions, will fill up the apparent gap between physiological processes which in a chemical sense are as widely separated from one another as those of oxidation and liberation of free oxygen.

J. S. HALDANE.



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THE PHOSPHORUS-CONTAINING SUBSTANCES OF THE CELL.

OUR knowledge of the form and position in which the phosphorus is held in the animal cell has of recent years become considerably advanced by the researches of Miescher, Kossel and their co-workers, and has been greatly stimulated by the discovery of Altmann, that from the chief and most abundant of the phosphorus proteids the phosphorus can be split off in the form of a complex acid which he has called *nucleic acid*. The constitution of nucleic acid has now been made the subject of a good deal of work which has led to all the more promising results, because though it is a very complex body, the size of its molecule is small as compared with those of the highly complex proteids by which it is accompanied. In this short paper we will first give an account of nucleic acid itself so far as it is at present known and then discuss the compounds in which form it is present in the cell.

NUCLEIC ACID.

Nucleic acid was first prepared and described by Altmann.¹ He obtained it best from yeast by treating the cells for about five minutes with a large volume of 3 per cent. NaOH. The alkali was then nearly neutralised with hydrochloric acid and an excess of acetic acid added. Most of the proteid was thus precipitated, whilst the

¹ Altmann : *Arch. für (Anat. u.) Phys.*, 1889, p. 524.

nucleic acid remained in solution and was obtained by adding hydrochloric acid up to 0·3 per cent. and then throwing it into an equal volume of alcohol containing the same amount of hydrochloric acid when the acid was precipitated. He also obtained similar bodies from thymus and egg-yolk. He showed that they all contained phosphorus varying in amount from 7·9 per cent. to 9·5 per cent. and that sulphur was absent if the acid had been completely separated from all proteid.

Nucleic acid as thus prepared was found to be a white amorphous powder giving a strong acid reaction. It dissolves fairly readily in water and very readily if a little ammonia or other alkali be added. From this solution it is not precipitated by an excess of acetic acid but is by weak hydrochloric, especially in the presence of alcohol. It is quite insoluble in alcohol and ether.

As early as 1874 Miescher¹ described the preparation and properties of a substance rich in phosphorus and of marked acid properties which he had obtained from the nuclei of salmon spermatozoa. This substance Miescher regarded as a nuclein, but as was pointed out by Kossel he was in reality dealing with nucleic acid. In his later work, collected and published by Schmiedeberg² after the author's death, Miescher recognises this and gives his further work upon the chemistry of the acid. Nucleic acid is particularly characterised by its instability and Miescher points out two great dangers which have to be avoided if we wish to obtain a preparation of unaltered nucleic acid. In the process of purification it is extremely liable to decompose, with the result that it loses a considerable part of its phosphorus. In the second place it is most easily split up in another manner in which it loses a considerable part of its nitrogen which is given off in the form of the so-called nuclein bases or alloxuric bodies, xanthin, hypoxanthin, etc. This latter decomposition occurs especially during that part of the preparation when it is necessary to employ dilute mineral acids,

¹ Miescher: *Verhandl. d. naturf. Ges. in Basel*, Bd. 6, S. 138, 1874, and *Ber. d. d. chem. Ges.*, Bd. 7, S. 1714, 1874.

² Miescher: *Arch. f. exp. Path. u. Pharm.*, Bd. 37, S. 100, 1896.

and though Miescher showed that it was easy to prevent the loss of any of the phosphorus, it was much more difficult to prevent the loss of any of the nitrogen. To avoid the latter source of error he found that it was necessary to keep the temperature of all solutions down to 0°C ., the whole time of the preparation, and particularly during the treatment with dilute acid, the time of which it is further necessary to reduce to the lowest possible limit. His method is to take the ripe spermatozoa and rub them up in water. The emulsion is then strained through a cloth, precipitated by adding a few drops of acetic acid, and filtered. The precipitate is washed thoroughly with alcohol and ether to remove fats, lecithin, etc., and then thoroughly shaken with 0.5 per cent. hydrochloric acid and this extraction repeated four or five times. It is in the later extractions that the danger of splitting off some of the nuclein bases is especially incurred. The residue is next dissolved in $\frac{1}{4}$ per cent. soda, filtered, and the filtrate acidified with hydrochloric acid when the nucleic acid is partially precipitated, but only completely on the addition of two volumes of alcohol. During the whole of these processes the preparation and all reagents added are cooled down to about 0°C . A general and striking reaction given by nucleic acid is that in acid solutions it will precipitate proteids, producing bodies which, as Altmann showed, very closely resemble if they are not identical with the nucleins obtainable from most tissues.

The importance of the part that nucleic acid plays in the animal economy, either when united with proteid molecules to form compound proteids or in a free state, stands forth clearly when we study the nature of the decomposition products which have been obtained from it, or, on the other hand, by a study of the positions from which it has been obtained. Thus its distribution is found to be chiefly, if not entirely, in the nuclei of cells. The chromatin of the nuclei appears to be the chief source of the acid, as is seen from Miescher's and other experiments in which the nuclei have been isolated in quantity and found to consist almost entirely of nucleic acid combined with a base, protamine. This conclusion is further strengthened by a study of the

reactions to staining agents. Thus on staining different substances with the Ehrlich-Biondi triple stain Kossel¹ found that proteids select the acid stain, *i.e.*, the fuchsine, that nucleic acid on the other hand takes the basic stain, the methyl-green. On staining resting nuclei with the triple stain they are tinted of a violet colour, and therefore Kossel argues contain the nucleic acid combined with proteid; on the other hand, in cells that are subdividing, the nuclei stain a green colour, showing that a large part of the nucleic acid is probably in a free state. The same reaction to stains is shown by the nuclei of freshly formed spermatozoa.

It is, however, with the constitution of nucleic acid that we are here chiefly interested, and our knowledge so far depends upon the decomposition products that have been obtained from it by various methods. The whole of the phosphorus may be separated as phosphoric acid by boiling it for a short time with dilute sulphuric acid. According to Liebermann² the phosphorus exists in the nucleic acid molecule in the form of metaphosphoric acid, for he obtains it as such by heating for a short time with dilute nitric acid. That it is not all held in the same way in the molecule is shown by the fact that one part of it can be readily split off from the molecule by simply heating it for a time in water. The major part, however, under such treatment remains unaffected.

Since Kossel first showed that the nuclein prepared from yeast yielded xanthin and hypoxanthin, and at a later time that the nucleic acid molecule was the source of these bases, many nucleic acids have been examined for these alloxuric bodies, so that now the identification of a nucleic acid depends upon proving the existence of one or more of these bases as a result of decomposition of the molecule. The chief members of this group of alloxuric bases are as follows :—

1. *Xanthin*, $C_5H_4N_4O_2$, which has been obtained from

¹ Kossel: *Deutsche. med. Woch.*, 1894, No. 7.

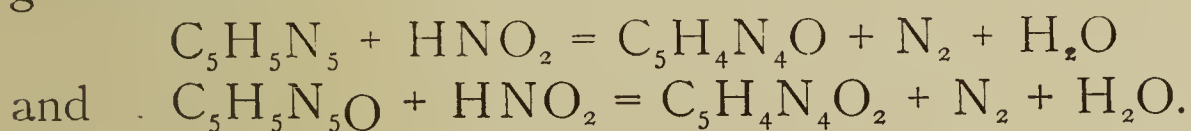
² Liebermann: *Pflüger's Arch.*, Bd. 47, S. 155, 1890; and *Ber. d. d. Chem. Ges.*, Bd. 21, S. 598, 1888.

very many organs, *e.g.*, from the nuclein of yeast (Kossel¹), from the nucleic acid prepared from ox-spermatozoa, or again from that of carp-spermatozoa. It has also been obtained from the spleen, the pancreas, the thymus, etc.

2. *Guanin*, $C_5H_5N_5O$, has been prepared from the nuclein of yeast (Kossel), and from the nucleic acid prepared from thymus (Kossel), and from that of salmon-spermatozoa (Miescher). It has also been obtained from numerous cellular organs, *e.g.*, liver, spleen, pancreas, etc. It is closely related to xanthin, into which it may be converted by the action of nitrous acid when an imido-group ($=NH$) of the guanin is split off and an O atom inserted in its place.

3. *Hypoxanthin*, $C_5H_4N_4O$, has been found in the same position as xanthin, and in especial quantity in salmon sperm (Miescher). It has been prepared from the nuclein of leucocytes (Kossel²) and of yeast (Kossel³).

4. *Adenin*, $C_5H_5N_5$, was first discovered by Kossel, who obtained it from the pancreas. He further obtained it from thymus-nucleic acid⁴ and from the nuclein of yeast. It stands in the same relationship to hypoxanthin that guanin does to xanthin.



The finding of these bases in this position is particularly of interest in conjunction with their close relationship to uric acid ($C_5H_4N_4O_3$), which only differs from xanthin in possessing one more oxygen atom. Their molecular structure too is similar, and just as with uric acid they are found to split up into alloxan, parabanic acid, etc., under the influence of oxidising agents. With regard to the part they may play in the formation of uric acid in the body we will return later.

Cytosin.—Associated with these bases though not showing exactly the same properties is another base,

¹ Kossel: *Ztschr. f. physiol. Chem.*, Bd. 4, S. 290, 1880.

² Kossel: *Ibid.*, Bd. 5, S. 152, 1881.

³ Kossel: *Ibid.*, Bd. 4, S. 290, 1880.

⁴ Kossel: *Ber. d. d. chem. Ges.*, Bd. 26, S. 2753, 1893.

cytosin, which Kossel¹ has obtained from thymus-nucleic acid in conjunction with adenin and guanin when the acid is boiled for twenty minutes in water. This base has a formula $C_{21}H_{30}N_{16}O_4 \cdot 5 aq$, and is precipitated by phosphotungstic acid, and forms crystalline salts with sulphuric and hydrochloric acids, and with platinum chloride or gold chloride. It is precipitated in the form of a silver compound by an ammoniacal solution of silver oxide. It was obtained to the amount of 2 per cent. of the nucleic acid from which it was derived.

We thus see that with regard to the nuclein bases they yield, the nucleic acids prepared from different organs have different constitutions, and Kossel suggests that for each base there is a corresponding nucleic acid, there being at least four of these. For these acids he suggests the names xanthylic, adenylic, etc. We cannot, however, as yet consider this as proven, the alternative that a nucleic acid may have one or more of different basic molecules within it being quite as probable, and, moreover, it explains the facts just as well as the other supposition. It is probable too that far greater differences lie in the other parts of the nucleic acid molecule than the variation in the alloxuric base with which it is combined. A further point of some importance is that though in these nucleic acids we are dealing with a compound of a body of distinct acid properties with a base, yet it is not of the nature of a salt. Kossel and Neumann² have shown that if thymus-nucleic acid be boiled in water for about five minutes the acid is split up into the nuclein bases, in this instance adenin together with smaller quantities of guanin and cytosin, and a phosphorous rich acid which they term *thyminic acid*. Thyminic acid contains no further nuclein base, but if an alloxuric base is added to its solution a thyminate of the base is found. This, however, differs from the original nucleic acid, for if the two substances are treated with barium hydrate and the resulting solutions precipitated by throwing

¹ Kossel: *Ber. d. d. chem. Ges.*, Bd. 27, S. 2215, 1894, and *Arch. f. (Anat. u.) Physiol.*, 1894, S. 551.

² Kossel and Neumann: *Zeitschr. physiol. Chem.*, Bd. 22, S. 81, 1896.

them into alcohol the filtrate in the case of the thyminate is found to contain the nuclein base, whereas in the case of the nucleic acid no base is split off.

Turning next to another group of bodies, it has been found that some of the nucleic acids yield *carbohydrates* among their decomposition products. Thus, working on the nucleic acid of yeast, Kossel¹ obtained evidence of a carbohydrate in that molecule, and in a later paper² describes the preparation of an osazone melting between 204° and 205° C. and of a second melting at 105° C. The first he shows to be due to a glucose which does not ferment with yeast, but which reduces Fehling's solution. The second osazone is probably due to a pentose as shown by the production of furfurol from it. It is to be noted that the carbohydrate molecule belongs to the nucleic acid part of the nuclein not to the proteid portion. He was not able to obtain any carbohydrate from the nucleic acid prepared from salmon spermatozoa. Hammarsten³ describes the preparation of a substance from ox-pancreas, very closely related to the nucleins, from which he was able to obtain a body reducing Fehling's solution by boiling the preparation with weak sulphuric acid. From the decomposition products he prepared an osazone melting at 158° to 160° C., easily soluble in alcohol or ether and which could be crystallised from its alcoholic solution by adding warm water and then allowing it to cool. The sugar did not ferment with yeast and its osazone did not correspond with that of any known sugar. It is probably a pentose. Another nucleic acid which has given evidence of the presence of a carbohydrate is that prepared from the thymus. On heating this for two hours in a Papin's digester at 150° C. with 20 per cent. sulphuric acid, Kossel and Neuman⁴ found levulinic acid among the bodies into which it had been decomposed; and

¹ Kossel: *Arch. f. (Anat. u.) Physiol.*, 1891, S. 181.

² Kossel: *Ibid.*, 1893, S. 157.

³ Hammarsten: *Zeitschr. Physiol. Chem.*, Bd. 19, S. 19, 1894.

⁴ Kossel u. Neumann: *Ber. d. d. chem. Gesell.*, Bd. 27, S. 2215, 1894; and Kossel: *Arch. f. (Anat. u.) Physiol.*, 1894, S. 536.

from the presence of this acid they infer the existence of a carbohydrate group in the original substance.

By obtaining nuclein bases we are able to separate some of the nitrogen of nucleic acid, but a large part still remains to be accounted for. In investigating thymus nucleic acid Kossel and Neumann¹ have shown that by dropping it into boiling water in about ten minutes it is split up into its nuclein bases and a new acid, *thyminic acid*. To isolate the latter the watery solution is cooled and an excess of barium hydrate added. It is then allowed to stand for twenty-four hours when a precipitate slowly forms, which consist of guanin mixed with some barium carbonate. This is filtered off and the filtrate added to twice its volume of alcohol when a white precipitate of barium thyminate is thrown down and the adenin and cytosin remain in solution. Barium thyminate is a very hygroscopic salt, easily soluble in water, to which they assign the formula $C_{16}H_{23}N_3P_2O_{12}Ba$. Thyminic acid, as prepared from this salt, differs in several important respects from the nucleic acid from which it has been obtained. In the first place it does not yield any nuclein bases on being boiled with dilute sulphuric acid. It is very soluble in water, whereas nucleic acid is only feebly soluble; it is not precipitated from its watery solution by dilute mineral acids whereas nucleic acid is, both give precipitates when added to an acetic acid solution of a proteid or propeptone, but the precipitate given by nucleic acid is only soluble with difficulty in dilute mineral acids, whilst that of thyminic acid is easily dissolved by hydrochloric acid or by many neutral salts. They consider that this thyminic acid is a paranucleic acid and is probably identical with the acids obtained from some of the paranucleins. It is important to note that in this acid the ratio of the nitrogen to the phosphorus atoms is 3 : 2, whereas in nucleic acid it is 6 : 2, so that by decomposing nucleic acid in this way one half of the nitrogen is split off in the form of nuclein bases.

Some of the nitrogen of the thymus nucleic acid, or thyminic acid, has also been obtained in the form of a

¹ Kossel u. Neumann: *Ber. d. d. chem. Gesell.*, Bd. 26, S. 2753, 1893; and *Zeitschr. f. physiol. Chem.*, Bd. 22, S. 74, 1896.

crystalline substance which has been named *thymin*. On boiling the nucleic acid with 30 per cent. sulphuric acid or heating it under pressure in water to 170° C. Kossel and Neumann¹ were able to isolate this substance from the resulting solution. It is only slightly soluble in cold but easily soluble in hot water. It is also dissolved by hot alcohol. It has a bitter taste, and on careful heating it sublimes. It possesses neither acid nor basic properties, is not precipitated by phospho-tungstic acid, but is by mercuric chloride, or an ammoniacal solution of silver oxide. Its formula is $C_5H_6N_2O_2$, and it is therefore isomeric with methyl-uracil. They also prepared it from the nucleic acids obtained from yeast and from the spleen. In working upon the nucleic acid obtained from salmon spermatozoa Miescher² also obtained a non-basic body of the formula $C_5H_6N_2O_2$, to which Schmiedeberg gives the name nucleosin. This body has also been prepared from the same source by Kossel,³ who has shown that it is identical with the thymin previously described by him.

Another body which has been obtained as the result of partial decomposition of the nucleic acid of yeast is a phosphorus-containing acid which Kossel⁴ terms *plasmic acid*. This was obtained as the result of the action of dilute alkalis on nuclein at ordinary temperatures. The proteid part of the molecule is gradually split off from the nuclein, and bodies richer in phosphorus are thus obtained, of which the most complex is plasmic acid. It differs from nucleic acid by its solubilities, being readily dissolved by water or dilute hydrochloric acid and can thus be easily separated from nucleic acid. Like nucleic acid, it precipitates proteids in acid solution, and thus closely resembles thyminic acid, from which, however, it differs, in that nuclein bases are found among its decomposition products after it has been

¹ Kossel u. Neumann: *Ber. d. d. chem. Gesell.*, Bd. 26, S. 2753, 1893; and Bd. 27, S. 2215, 1894. See also *Arch. f. (Anat. u.) Physiol.*, 1894, pp. 194, 536 and 551.

² Miescher: *Arch. f. exp. Path. u. Pharm.*, Bd. 37, S. 125, 1896.

³ Kossel: *Zeitschr. f. physiol. Chem.*, Bd. 22, S. 188, 1896.

⁴ Kossel: *Arch. f. (Anat. u.) Physiol.*, 1893, p. 157.

boiled with dilute sulphuric acid. It shows a further difference from the nucleic acid from which it is obtained by yielding no carbohydrate molecule. It has not at present been prepared from nucleic acids derived from other sources. Its formula is given as $C_{15}H_{28}N_6P_6O_{30}$, and thus shows a relatively higher amount of phosphorus compared to nitrogen than either nucleic or thyminic acids.

We see then that nucleic acid is a very complex body which is moreover characterised by the ease with which it can be broken down into simpler and more stable molecules. Hence its preparation for purposes of analysis requires considerable care, and the formulæ which have been given for it can only at present be regarded as approximate. Miescher's formula for "salmonnucleic acid" as recorded by Schmiedeberg¹ is $C_{40}H_{54}N_{14}P_4O_{27}$ and for a sample of yeast nucleic acid prepared by Altmann, Miescher² obtained the formula $C_{40}H_{54}(OH)_5N_{14}P_4O_{27}$ which only differs from that of salmonnucleic acid by 5 (— OH). The formulæ given by Kossel are somewhat different. Thus for nucleic acid from yeast he gives³ either $C_{17}H_{26}N_6P_2O_{14}$ or $C_{25}H_{36}N_9P_3O_{20}$, and for that from the thymus⁴ $C_{30}H_{52}N_9P_3O_{17}$. Recently Mathews⁵ working under Kossel has prepared a nucleic acid from the spermatozoa of an Echinoderm, *Arbacia*, and finds that the analyses of this agree completely with the later formula given by Miescher for that from salmon spermatozoa.

THE COMPOUNDS OF NUCLEIC ACID FOUND IN DIFFERENT CELLS.

Having considered the structure of nucleic acid we may next examine the different combinations in which it is found in different cells. In different positions they are found combined with proteids, or with bases belonging to the group

¹ Miescher: *Arch. f. exp. Path. u. Pharm.*, Bd. 37, S. 121, 1896.

² *Loc. cit.*, S. 122.

³ Kossel: *Arch. f. (Anat. u.) Physiol.*, 1891, S. 181.

⁴ Kossel: *Ibid.*, 1893, S. 157.

⁵ Mathews: *Zeitschr. f. chem. Physiol.*, Bd. 23, S. 399, 1897.

of protamines or they exist in the free state. Their simplest compounds with proteids form the group of the *nucleins*, a name first applied to the substance isolated by Miescher from the nuclei of pus cells, being the residue left on submitting the cells to gastric digestion. The nucleins are found to be present in nearly all cells, though not in a free state, but combined with a further amount of proteid to form the group of bodies termed the *nucleo-proteids*. The nucleo-proteids react as weak acids whose neutral alkaline salts are soluble in water and coagulate on heating. If the solution or the heat coagulum be digested with artificial gastric juice most of the proteid is split off and dissolved leaving an insoluble residue, the nuclein. From the nuclein, as Altmann showed, we can, by the use of alkali, split off a further amount of proteid and thus obtain the nucleic acid. The nucleo-proteids are at present regarded as the most characteristic proteids obtainable from nuclei.

Closely related to the nucleins is another group of bodies containing phosphorus and showing many other points in common but differing in that they do not contain any nuclein base. To this group of bodies Kossel gives the name of *paranuclein* but for them Hammarsten¹ suggests as a more satisfactory term the name *pseudo-nuclein*. As the nucleins are found in the cell or nucleus combined with proteids as nucleo-proteids, so the paranucleins are also found combined with proteids. To these proteids Hammarsten suggests that the name of *nucleo-albumins* should be limited. Examples of the nucleo-albumins are casein and ovo-vitellin.

On artificial gastric digestion they leave an insoluble residue of paranuclein, but if the digestive fluid be strong the paranuclein gradually dissolves. The amount of phosphorus found in solution varies with the strength of the digestive fluid and with a smaller amount of the nucleo-albumin submitted to digestion. As a nuclein is split up into nucleic acid and a proteid so a paranuclein breaks down into a proteid and paranucleic acid, but this latter has not yet been much examined. Kossel and Neumann consider

¹ Hammarsten : *Ztschr. f. physiol. Chem.*, Bd. 19, S. 19, 1894.

the thyminic acid which they have isolated by removing the nuclein bases from thymus nucleic acid to be a paranucleic acid, but this cannot be considered proved until we know more about the solubilities and decomposition products of paranucleic acids prepared directly from casein and similar bodies.

The nucleo-albumins in contradistinction to the nucleoproteids are apparently contained in the cell protoplasm and behave as strong acids, insoluble in water but readily dissolving in weak alkali. A neutral solution of their alkaline compound does not coagulate on heating and they apparently always contain iron and less sulphur than ordinary proteids.

In a recent paper Milroy¹ has examined the bodies obtained by precipitating acid solutions of proteids with nucleic and thyminic acids. The former Altmann considered as identical with the nucleins, and Milroy shows that these artificial nucleins and paranucleins give many of the reactions of the true nucleins and paranucleins, *e.g.*, their solubilities, percentage of phosphorus and behaviour to digestive ferments are the same; this resemblance however, is not sufficient to establish complete identity.

PROTAMINE.

Another compound of nucleic acid is with the *protamine* bases. So far these have only been found in the nuclei of the spermatozoa of fishes. The protamines are nitrogenous bases, only found at present in this position, and have recently been considerably studied in this connection.

The nucleic acid in the nuclei of the salmon spermatozoa appears to be present combined with a base which Miescher² termed protamine, and to which he assigned the formula $C_9H_{21}N_5O_3$. It forms about one-fourth of the total weight of the dried spermatozoa heads, and was isolated from them by repeated extraction with dilute hydrochloric acid (about

¹ Milroy: *Ztschr. f. physiol. Chem.*, Bd. 22, S. 307, 1896.

² Miescher: *Verhandl. d. naturf. Gesell. in Basel*, Bd. 6, S. 138, 1874; and *Ber. d. d. chem. Ges.*, Bd. 7, S. 396, 1874.

5 p.m.). The extract was then neutralised and platinum chloride added, when an amorphous precipitate of protamine platinum chloride is obtained. The free base possesses marked basic properties; is soluble in water but insoluble in alcohol or ether. It is precipitated by phospho-tungstic acid, potassio-mercuric iodide, mercuric chloride and gold chloride. The examination of this body was continued by Piccard,¹ who found that Miescher's preparation contained small quantities of guanin and hypoxanthin, and by fresh analysis of the purified platinum salt obtained the formula $C_{16}H_{32}N_9O_4$.

Twenty years later Kossel² described an important reaction of protamine, for he found that in an ammoniacal solution of a proteid or albumose protamine gave a precipitate which showed all the reactions of *histon*, a proteid which he had previously obtained³ from the red corpuscles of birds' blood.

In Miescher's later paper further analyses of protamine are given from which the formula $C_{16}H_{28}N_9O_2$ is obtained. To investigate the constitution of protamine he heated the hydrochlorate up to 170° C. in a sealed tube with 15 per cent. hydrochloric acid, and among the decomposition products was able to isolate the base *Arginine* $C_6H_{14}N_4O_2$, which, originally discovered by Schultze and Steiger⁴ in vegetable tissues, has since been isolated by Hedin⁵ from the decomposition products of many proteids of animal tissues. The great interest of this base lies in the fact that if its silver compound be boiled with baryta water for twenty minutes it yields urea. Hedin also shows that the base lysatine obtained by Drechsel from proteids, which similarly yields urea, is in reality a mixture of lysine with arginine.

¹ Piccard: *Ber. d. d. chem. Ges.*, Bd. 7, S. 1714, 1874.

² Kossel: *Deutsche med. Wochenschr.*, 1894, No. 7.

³ Kossel: *Zeitschr. f. physiol. Chem.*, Bd. 8, S. 511, 1884.

⁴ Schultze and Steiger: *Ztschr. f. physiol. Chem.*, Bd. 9, S. 43; and *Ber. d. d. chem. Ges.*, Bd. 24, S. 2707; and Bd. 29, S. 352, 1896.

⁵ Hedin.: *Ztschr. f. physiol. Chem.*, Bd. 20, S. 186, 1895; Bd. 21, S. 155, 1896; and Bd. 22, S. 191, 1897.

The method Kossel¹ recommends for the preparation of protamine is to thoroughly extract the dried spermatozoa heads, which have been previously freed from fat, etc., by extraction with alcohol and ether, with 1 per cent. sulphuric acid. This is then filtered and added to three times its volume of alcohol, when a precipitate of protamine sulphate is formed and can be readily purified. From analysis of the pure salt he arrives at the formula $C_{16}H_{31}N_9O_3$ for the base. He criticises Miescher's formula, showing that it cannot be correct because it does not satisfy the necessary condition that the sum of the valencies should be an even number, and points out that if another H atom be added to Miescher's formula, then the two only differ by one molecule of water. For the protamine obtained from salmon sperm he suggests the name *salmine*, and for a similar protamine obtained from the spermatozoa of the sturgeon, the name *sturine*. If sturine be decomposed by boiling it for eight hours with 30 per cent. sulphuric acid and then the sulphuric acid be removed, an alkaline fluid is left which gives a precipitate with mercuric chloride, leaving further bases still in solution. The mercuric precipitate is a compound of a new base which Kossel terms *histidine* having a formula $C_6H_9N_3O_2$, which is readily soluble in water, but crystallises out when an equal volume of alcohol is added to its solution. It is quite insoluble in ether. Histidine has also been found by Hedin² among the decomposition products from casein, egg-white and vitellin. From the filtrate from the histidine precipitate Kossel also prepared arginine, thus confirming and extending the work of Miescher upon salmine. Kossel further examined the decomposition products of sturine for *amido-acids*, but found that they were only present in quite minimal amounts, so that practically the whole of the nitrogen of the protamines is present in the form of bases. Balke³ showed that protamine resembled the alloxuric bodies in that it gave a white precipitate with cuprous

¹ Kossel: *Zeitschr. f. physiol. Chem.*, Bd. 22, S. 178, 1896; and *Stzber. d. Preuss. Ak. d. Wiss.*, Bd. 18, S. 403, 1896.

² Hedin: *Ztschr. f. physiol. Chem.*, Bd. 22, S. 191, 1896.

³ Balke: *Journ. f. prakt. Chem.*, Bd. 47, S. 537, 1893.

oxide from an alkaline solution. Thus, if hydroxylamine hydrochlorate is added to a solution of protamine and then a few drops of Fehling's solution, the cupric hydrate is reduced to the cuprous, and this gives a white precipitate with the protamine. Balke also showed one other very interesting reaction of protamine, for he found that it gave the biuret reaction. This has been confirmed by Miescher and by Kossel for salmin and sturin. As Kossel points out, sturin resembles the peptones in some respects, but differs from them in not yielding any of its nitrogen in the form of amido-acids. Protamine gives neither the xanthoproteic nor Millon's reaction. Kossel suggests that the biuret reaction in those cases where it occurs is due to the basic constituent of the proteid molecule. In a later paper Kossel¹ describes the preparation of a protamine from the spermatozoa of the herring. This one he names *clupeine*, and assigns to it the formula $C_{30}H_{57}N_{17}O_6$.

In his examination of the spermatozoa of *Arbacia* Mathews² prepared a protamine which he has termed *arbaccine*, which shows differences from those previously examined. The sulphate was prepared according to the method employed by Kossel, and from this the free base was obtained. It gives an alkaline solution in water, gives the biuret reaction, and gives no precipitate with Millon's reagent, though the fluid turns red in boiling. Like the other protamines it precipitates proteids or Witte's peptone in alkaline solutions, but it again differs from them in the lower amount of nitrogen it contains. Mathews finds that in the spermatozoa it is combined with the nucleic acid; that there is no free nucleic acid present. The chromatin apparently consists entirely of arbaccine nucleate. In examining the ripe spermatozoa of the herring he shows that the chromatin in all probability again consists entirely of clupeine nucleate, clupeine being the protamine which Kossel lately described as prepared from the herring. He further confirms the statements of Miescher and Kossel that

¹ Kossel : *Sitz. d. Ges. z. Beford. d. g. Naturwiss. in Marburg*. July, 1897.

² *Loc. cit.*

no protamine is to be obtained from the spermatozoa both of oxen and hogs. It is remarkable to find that the chromatin of such cells as the spermatozoa should chemically consist of substances which relatively to proteids are of simple constitution. If it be true that hereditary characteristics are transmitted by the chromatin of the reproductive cells we should have expected a most complex chemical structure for these parts, and it therefore becomes the more striking to note that the most complex protamine obtained, arbacin, is from the animal lowest in the scale, and that in the higher vertebrates examined no protamine is present at all.

THE FORMATION OF URIC ACID.

Following on his discovery of a group in the nuclein molecule, which yielded alloxur-bases, Kossel suggested that the nucleins might prove the source of the uric acid excreted by animals. The first direct evidence in favour of this view was, however, first supplied by Horbaczewski¹ who found that if spleen pulp be mixed with blood and a stream of air passed through it, on keeping this at body temperature for a few hours the xanthine bases gradually disappeared and uric acid appeared in their place. The experiments of giving the alloxur-bases as drugs to animals has not led to definite results in this direction, but in experiments with nuclein or nucleo-proteids the results in the case of most observers have been in the direction of a proportionate increase in the uric acid out-put. In a recent paper, Jerome² narrates the result of an experiment upon himself in which the uric acid excretion was determined upon different dietaries. He concludes that "the daily out-put of uric acid is so easily, so surely and so largely controlled by the use of suitable articles of diet as to make it highly probable that the variations in the amount of uric acid excreted in health from day to day are

¹ Horbaczewski: *Monatsh. f. Chem.*, Bd. 10, S. 624, 1889; and *Ztschr. f. Physiol. Chem.*, Bd. 18, S. 341, 1893.

² Jerome: *Jour. of Physiol.*, vol. 22, p. 146, 1897. (This paper contains an account of previous work upon this subject.)

chiefly due to the larger or smaller quantity of alloxur-holding bodies absorbed from the food: chiefly, but not wholly, for there are other influencing conditions the effects of which have yet to be worked out”.

According to Horbaczewski, the production of uric acid after the ingestion of nucleins is not derived from the alloxur-bodies taken, but rather depends upon the leucocytosis which they induce, *i.e.*, that it is produced chiefly from the nuclein of the leucocytes as they break down. Thus he points out that the amount of uric acid excreted varies directly with the degree of leucocytosis which had immediately preceded it, so that a direct relation between the two seems to be definitely determined. Mares,¹ however, objects to this view that it is by no means proved that the uric acid results from disintegration of the leucocytes, but rather that it may arise from the metabolism or increase in rate of formation of the cells.

PHOSPHOCARNIC ACID.

In some respects allied to the nucleins is a phosphorus-containing acid obtained by Siegfried² from muscle and named by him phosphocarnic acid. This is contained in the watery extract from muscle and is obtained from it by first precipitating the phosphates by calcium chloride and ammonia, and then, while boiling, ferric chloride is gradually added, the reaction during this stage being kept weakly acid. In this way the iron salt of phosphocarnic acid, carniferrin, is precipitated and the constancy of the analyses of several preparations both of carniferrin and the free acid prepared from it proves that we are dealing with one body only, which is of constant composition. By boiling with dilute mineral acids phosphocarnic acid is split up into a carbohydrate which reduces Fehling's solution, carbonic, succinic, paralactic and phosphoric acids and in addition a new acid called *carnic* acid. The nature of the carbohydrate has

¹ Mares: *Wien. Sitzungsber*, Bd. 101, Abthg. iii., 1892.

² Siegfried: *Ber. d. d. Chem. Ges.*, Bd. 28, S. 515, 1895; and *Ztschr. f. physiol. chem.*, Bd. 21, S. 360, 1896.

not yet been determined. Carnic acid¹ is of some considerable interest. It is free from sulphur having a formula $C_{10}H_{15}N_3O_5$; it gives the biuret but not Millon's reaction. It forms an addition compound with one molecule of hydrochloric acid in which the chlorine is not precipitated by silver nitrate until it is boiled with nitric acid. It thus gives all the reactions of antipeptone with which Siegfried considers it identical, a view which is confirmed by Fränkel² who finds that antipeptone when prepared pure by the artificial digestion of proteids is sulphur free. Siegfried compares phosphocarnic acid to the nucleins, pointing out that where the nucleins yield proteid phosphocarnic acid yields antipeptone and he therefore suggests for it the name nucleon. Siegfried considers that the muscle-nucleon or some compound of it plays an important part in the metabolism of muscle during its activity, for he finds that its amount is much less in fatigued than in resting muscle and points out that the formation of carbonic acid, phosphoric acid, and lactic acid during a muscle's activity can be at once explained as the result of the decomposition of phosphocarnic acid. This view is further strengthened by the experiments of Krüger³ who shows that muscle-nucleon on hydrolysis gives off considerable quantities of carbonic acid and that no other extract from muscle does this. For this production of carbonic acid no oxygen is required.

Phosphocarnic acid has also been prepared from the urine⁴ where it is present in very small quantities and also in milk.⁵ The nucleon obtained from the latter source is however a slightly different structure, for it yields on decomposition fermentation lactic acid in the place of paralactic. From the fact that it yields antipeptone on decomposition

¹ Siegfried: *Ber. d. k. Sächs. Ges. d. Wiss. zu. Leipzig. Math.-Phys. Classe.*, 1893, S. 485; *Arch. f. (Anat. u.) Physiol.*, 1894, S. 401, and *Ber. d. d. chem. Ges.*, Bd. 27, S. 2762, 1895.

² Fränkel: *Wiener. Med. Blätt.*, 1896, S. 703.

³ Krüger: *Ztschr. f. physiol. Chem.*, Bd. 22, S. 95, 1896.

⁴ Rockwood: *Arch. f. (Anat. u.) Physiol.*, 1895, S. 1.

⁵ Siegfried: *Loc. cit.* and *Ztschr. f. physiol. chem.*, Bd. 22, S. 575, 1896. Wittmaack, *ibid.*, Bd. 22, S. 567.

with dilute acids and that it offers a possible explanation of the production of some of the waste products of muscular activity, muscle-nucleon becomes a body of considerable importance and interest, though fresh confirmatory work is required before we can consider its *rôle* as a store of muscular potential energy at all satisfactorily determined. One fact which seems to bear against this view is that the amount of it present in muscle is very small, *viz.*, from 0·1 to 0·2 per cent. Its iron compound carniferrin has been shown by Hall¹ to be readily absorbed in the small intestine. It may thus to some extent serve as one means by which iron is taken into the body.

T. G. BRODIE.

¹ Hall: *Arch. f. (Anat. u.) Physiol.*, 1894, S. 455.

JULIUS SACHS.¹

Si quis tota die currens
Pervenit ad vesperam satis est.

AFTER great suffering Julius Sachs sank peacefully to rest at six o'clock on the morning of 29th May, 1897, at Würzburg, the scene for many years of his labours. Wherever scientific botany has a home, and by many outside the narrow circle of specialists, this loss has been regarded as irreparable. By no one has it been felt more keenly than by the writer of these lines, who will always thankfully recall the happiness it has been to him to have been closely connected throughout a long series of years as pupil and friend with him who has passed from our midst.

When I attempt to briefly sketch the life of the man to whose brilliant intellect botany is so greatly indebted, there rises involuntarily to my mind the saying of Petrarch's that I have quoted above, a saying at once so sad and yet so consoling.

Yes, his life was a struggle, a ceaseless, single-minded pressing-forward without rest to the goal of knowledge. To him study, research, teaching were not merely the external activities of his calling that might be laid aside for hours, days or even weeks, and then be again resumed. They absorbed his whole being more than was good for his personal welfare. But the evening came after this long day in which he had so faithfully laboured. No one realised this more fully than he himself. A prey to physical suffering, his sharpest pang was that he could no longer work for science with his former energy, and if anything made it hard for him to face death, it was the knowledge that he must leave behind as an unfinished sketch much that he wanted to say to the world.

¹ This article, which is somewhat shortened, is a translation by Miss E. D. Shipley of Professor Goebel's article in *Flora oder Allgemeine Botanische Zeitung*, 1897.

He had been chiefly occupied during these last years with a work which, under the title of *Principien Vegetabilischer Gestaltung* (*Principles of Vegetable Form*), was to set forth his views upon causal morphology. "I should feel it an immense grief if I were prevented from writing this book," he says: "it would embody the thought of forty years, and it is always important that one's ideas should be long and thoroughly brooded over. To finish it would render the last years of my truly miserable existence in some degree bearable."¹

We shall refer again to the purposes of this book, and turn now to a short account of Sachs' career.

He was essentially a "self-made man," who found it by no means a light matter to attain the eminence which led the most distinguished German universities each to desire to win him for itself. The story of his early years as it appears in these pages is taken from an autobiography intended for his own family, Fräulein M. Sachs having kindly made extracts from it for my use. It will be of great interest to many who only knew him as a mature man occupying an honourable position to learn how literally true were the words "tota die currens".

Sachs was born on 2nd October, 1832, at Breslau, where his father was an engraver. For a time his parents lived in the country, and this may have contributed to the early awakening of his mind to the beauty of nature at which he always looked as much with the eye of an artist as with that of an observer. The design that he cherished at one time of writing a work on the beauties of the plant-world was unfortunately never realised. It would have been of the greatest interest if he, an adept in the art of word-painting, an enemy to all affectations and mannerisms, had given us his thoughts upon this theme.

His first experiences of school life were not pleasant. Learning by heart, that purely mechanical acquisition of knowledge, was a burden to him as it has been to many another highly gifted scholar. Of much greater importance

¹ The quotations are principally taken from letters.

than his school instruction was his father's training in drawing. From his thirteenth to his sixteenth year he drew and painted flowers, fungi, and other natural objects, and his artistic talents played, as we shall see later, an important rôle in his career.

His family possessed but few books and the boy felt stirring within him a longing, doubtless inexplicable to himself, for intellectual advantages. And thus his brother's acquaintance with the sons of the physiologist, Purkynje,¹ at that time a professor at Breslau, was of great importance to him. His brother brought home the *Penny Magazine* from these playfellows, and the prehistoric animals depicted in it aroused so great an interest in Julius, then as always thirsting for knowledge, that although he could not understand the English text the "extinct monsters" appeared to him most realistically in his dreams! Later he himself came to know Purkynje's sons and this acquaintance shed a ray of light upon his life; for the first time he saw a refined home, free from all petty cares as to daily bread, filled by stirring intellectual life, and dominated in every detail by the imposing figure of the white-haired professor who inspired Sachs with the greatest respect. Julius learned to press plants from his sisters and heard that there were such things as botanical collections: he proceeded to start one for himself. His father, who knew the popular names of many plants, encouraged these endeavours. They made expeditions in the early morning hours, and at fourteen years old Sachs could already determine his plants according to Scholtz's *Flora*. But his herbarium was stolen, and this was his first bitter, deeply felt grief. He related his loss to every one and could not understand that other people failed to recognise its gravity. He never again collected plants until in later years, as professor, he started an herbarium for the purposes of demonstration. The way in which at the present day so many botanists entirely neglect

¹ J. E. Purkynje (1787-1869) was Professor of Physiology and Pathology in Breslau from 1823 till 1850, and afterwards in Prague. He was the author also of a botanical treatise (*De cellulis antherarum fibrosis nec non granorum pollinarium formis commentatio phytotomica*, Breslau, 1830).

the practical knowledge of plants was wholly distasteful to him, as the following remark in one of his letters shows. "I strongly disapprove of the so-called 'physiologists,' to whom the commonest meadow and garden flowers are unknown, especially as such people generally have but little knowledge of physics." And if he complained many a time in joke of the foolishly unnecessary and tedious multiplication of phanerogamic varieties he was far from undervaluing the knowledge and study of them. Indeed we shall come across instances of the keen interest in the common problems of systematic botany which constantly appears in his writings.

It was his mother who conceived the thought of allowing him to attend the gymnasium, a privilege accorded to none of his brothers, for this considering the family poverty involved no slight risk.

The years he spent at the Elizabeth Gymnasium formed a bright picture in Sachs' life. The school work was congenial to him, it lifted him out of the petty surroundings of his home-life into a higher sphere. He attended the gymnasium from 1845 to 1850. Of the masters only one—Dr. Rumpelt—came at all into personal contact with him. He recognised Sachs' exceptional talents and the two became good friends. On the other hand the natural science master, the lichenologist Körber, only repelled him. Körber could not instruct and had no conception how to impart anything worth knowing about his subject. Sachs therefore worked on at his scientific pursuits unaided and undirected. He read eagerly, without its doing him any harm, Oken's *Philosophy of Nature* which he had bought at a sale for a few pence, began to make a collection of skulls, and wrote a monograph on the crayfish. Körber's attention was drawn to this work by Dr. Rumpelt; he sent for Sachs and solemnly warned him against devoting himself to natural science, on the ground that it would not bring him in a half-penny! One cannot but rejoice that this advice was not acted upon.

In the year 1848 Sachs lost his father, and, in the following year, his mother. Thus orphaned, he lived at first with his brother, where, to his great joy, he was

allotted a room in the roof which, although otherwise unattractive, afforded him the opportunity of carrying on his scientific studies in his scanty leisure. Here, for instance, he mastered the Latin Anatomy of Bartholinus. It became more and more imperative however that he should face his position. He left the school (having risen to the upper second form) and wished to go to sea.

In the meantime Purkynje had been called to Prague. He remembered his son's friend and wrote suggesting that Sachs should come to him as a kind of private assistant. He was to prepare natural science drawings and in return to receive the modest salary of 100 florins a year and his keep.

After numerous difficulties with his guardians, Sachs left Breslau on 14th February, 1851, for Prague. He found there shelter, it is true, but no home. Purkynje was a man of high attainments, for whose genius Sachs had great respect. But their peculiar temperaments made it impossible for them to understand each other, and the elder naturalist had no word of recognition, sympathy or encouragement for the younger. He was of peasant origin and this stuck to him all his life. Sachs, on the other hand, felt himself—as he said with reason, in spite of the reduced circumstances of his family—to be a born aristocrat, and so there could not fail to be friction between them.

Whilst Sachs was at Prague, the question arose whether he should remain simply an illustrator of scientific writings or should carry on his studies further. Fortunately he decided upon the latter course, and despite the time that had elapsed since he left school, successfully passed his matriculations at Prague in the autumn of 1851 with a view to entering that university.

The young student was already too independent and critical to be an ardent frequenter of the lecture room, where it would have required a man of exceptional ability to have secured his attendance, and it was evident that there were at that time but very few such men at the University of Prague. Botany was represented by Kosteletzky, who was lecturing upon Schleiden's works. Sachs attended two or

three lectures and then stayed away ; the truth was that he needed no teaching on this subject. He paid special attention to chemistry, physics and mathematics. But the only man who attracted and helped him on was Robert Zimmermann,¹ who invited him to his house. "I went to him with an inclination towards philosophy but he directed me into the right way," Sachs says, speaking of Zimmermann ; "he and any earlier teacher Rumpelt are the only two who gave me any real help, apart from their aid I am self-taught". He read a good deal of philosophy after he had become acquainted with Zimmermann,—Herbart, Leibnitz, Kant, Locke, Hume and even the Schoolmen. At the same time he was privately working at zoology and botany, and for several years paid special attention to physics and mathematics. In 1856 he was made Doctor of Philosophy, a degree which at that time was hard to obtain at Prague. His outward circumstances, since he had separated from Purkynje, remained precarious ; he earned small sums by literary work, drawings of fossils, etc., and at this time made his first experiments in the physiology of plants. In 1857 he was made Privat-docent in Plant Physiology. Up to that time this had not been a recognised subject and there were various difficulties to overcome. "Two lectures are ample for all there is to say upon the physiology of plants" said Rochleder, the chemist, and at that time he was not so very far wrong.

Sachs, who later was certainly the best teacher that the new botany has produced, was by no means a success as Privat-docent. One reason of this may be that he took but slight interest in the art of teaching. He lived wholly for science and was beyond measure studious, "it engrossed my thoughts even when I was out walking," he says. This being so, it came to him, according to his own account, more or less as a revelation that what he had to do was not only to acquire as much knowledge as possible but also to produce some original work. From that time he only

¹ Robert A. Zimmermann, born at Prague, in 1824, studied philosophy, mathematics and natural science, became Professor of Philosophy at Prague in 1852 and since 1861 has held the same chair at Vienna.

sought to work out his own ideas, to attain his own aims. He became acquainted with several of the chief exponents of botany of the day, such as Unger, Nägeli, and Alexander Braun, all of whom he met at the Natural Science Congress in 1856 at Vienna; and also about 1857 with Hofmeister who, in the intercourse that lasted between them for many years, influenced Sachs strongly, though, as the latter considered, at times in such a way as to perplex him.

In the meanwhile he was finding his life in Prague almost unbearable. The patriotic Czechs of the National party opposed him as a German, and openly told him that they wanted to drive him away. Whilst this was going on, the attention of Prof. Stein, the well-known zoologist, had been directed to Sachs. Stein had formerly devoted some of his time and energy to the Academy of Forestry at Tharandt and introduced Sachs to the chemist Stöckhardt, the director of this institution. Sachs was invited to draw up a statement as to the relation of plant-physiology to agriculture, with the result that he was called to Tharandt as physiological assistant in 1859. He went there in the March of that year. His chief work here was to show that land plants could be raised in aqueous solutions of nutrient salts, but he was busy at the same time with other physiological experiments. "Die entdeckungen lagen damals am Wege" was his opinion, "die Botaniker trieben audere Dinge". Even then Nägeli, for instance, described Sachs' researches as belonging to the chemistry of agriculture, there was as yet no talk in Germany of the chemistry of plant-physiology.

In summer he started work at four o'clock in the morning, and by so doing found time during the years 1859 and 1860 to study the earlier plant physiologists besides doing his own work. These literary studies caused him in 1860 to suggest to Hofmeister that they should edit a large hand-book of botany, in which the collected results of what we now call "general" botany should be critically set forth. The *Handbuch der Physiologischen Botanik* remains, as is well known, a fragment: various collaborators who had undertaken certain parts drew back,

and Hofmeister fell ill and died in 1877 without being able to complete his share ; but in spite of all mishaps the four volumes that appeared rank among the most valuable productions of more recent botanical literature. Sachs had to frequently give addresses at agricultural meetings and so gained the useful knowledge that he had a natural gift for public speaking.

In the winter of 1860-1 he was invited to become the head of the recently established agricultural department of the polytechnic at Chemnitz. His position there bristled with difficulties, and he welcomed the proposal that he should accept the Chair of Botany and Natural History at Poppelsdorf near Bonn, whither he removed in 1861. Here he married and in time became the father of two daughters and a son.

As regards science the six years spent at Bonn are amongst his most fruitful. Besides a number of other works it was here that his *Experimental Physiology* was written and the *Text-book* begun. His lectures were highly appreciated and at the end of two years he was relieved from lecturing upon mineralogy and zoology ; henceforward he dealt only with physiology during the winter, and in the summer delivered special lectures on agricultural plants. There was but little intercourse between him and the botanist Schacht, who was then at Bonn, but who was already in bad health, and whose temperament was thoroughly uncongenial to his own. With Schacht's successor, Hanstein, on the contrary, friendly relations ensued. On New Year's Eve, 1866, he received the news that he had been called to Freiburg im Breisgau as successor to De Bary : he went there in April, 1867. A small salary and a poor garden formed two undesirable elements in his life at Freiburg, and after three terms he willingly left to go to Würzburg. There, as we know, he remained, in spite of brilliant offers to move elsewhere. As early as 1869 he received a call to Jena, in 1872 to Heidelberg, in 1873 to Vienna, in 1877 to Berlin, where later they tried to obtain him for the Agricultural College ; he was also invited to Bonn under tempting circumstances. When

Nägeli retired, the professorial chair at Munich was offered him. It is much to be regretted that he did not accept one of these invitations whilst his health was still good, especially as the climate of Würzburg is hardly favourable to nervous constitutions. It may perhaps have been the needs of his family which pressed heavily upon him, or attachment to all he had acquired at Würzburg and dislike to the loss of time and strength inseparable from each change of place, that kept him there. The Government testified its appreciation by investing him with titles and orders, as early as the autumn of 1871 his colleagues chose him for their rector, and he was repeatedly elected to the Senate.

With the commencement of his professorial life at Würzburg, Sachs' "Wanderjahre" came to an end. They had been, as the preceding facts show, beset with difficulties. "I was thirty-six years old when, with a salary of about 2000 gulden I came to Würzburg and found a hole in which to hide my head. During the three previous years in which I had laid aside the *Experimental Physiology* and had been writing the *Text-book*, I had had a severe struggle in the strictest sense of the word to provide for the wants of my family. I was thirty-seven years old when I succeeded for the first time in investing 200 thalers in the public funds, and had for twenty years daily worked from fourteen to fifteen hours. As you see, my life has not been an easy one, and yet I wish that things went as well with me now as they did then, for what I have been through since is truly more than a man can bear."

The strong expression that he uses in speaking of the laboratory at Würzburg shows that there was much to be desired both in it and in the gardens attached to it. The laboratory which under his direction obtained a world-wide reputation and attracted young botanists from all parts, was housed, together with the clinical schools and the Institute of Pharmacology, in a building that contrasts most modestly with the handsome modern structures that have arisen in many universities. And yet how much he accomplished in it! Little by little the whole of it came to be given up to botanical purposes, Sachs being much too modest to insist

on a new botanical laboratory in spite of the fine new buildings that were erected for the other sciences. He contented himself with the addition of a very beautiful and suitable lecture-room. He was particularly anxious about the garden, which was laid out on barren soil made up chiefly out of the rubbish-heap of an old fortress *glacis*. He gave it his own personal and devoted attention, and was rewarded by a luxuriant vegetation where formerly there had been but a barren waste. Later on he divided off a small part of the garden for special purposes and this he attended to himself with the help of his laboratory servant. There he made open-air experiments, and there also was the well-known *Schilderhaus* (sentry-box) for experiments in etiolation, etc. The cultivation of strong, healthy plants for the purposes of investigation was in his opinion an essential part of experimental physiological work ; he excelled in the art and deemed it worthy of individual, personal attention. There were almost invariably plants growing in his work-room, but in summer time, when growth was going on in the plant-world, it was essential to him to make constant observations out of doors and to meditate upon his investigations as he strolled about the garden.

The astonishing amount of work that he managed to get through from his earliest days could not but affect his constitution. He said himself that he had paid for each of his books with wearisome ill-health, and even the strongest nerves could not stand such ceaseless labour. Added to this came his wife's long tedious illness which undoubtedly helped to undermine his strength.

Bearing these facts in mind it is perhaps more possible to form a just estimate of his relations with the outer world. The latter part of his life found him a lonely man who had estranged many of his friends by bitter and sometimes even unjust criticisms. We shall perhaps condone his trenchant animadversions upon the botanical writings of his day if we remember how his sensitive, highly strung temperament must have suffered at times from the irritation of private affairs. And then again, science represented to him all that is highest in life, and it followed that any work which

he considered bad from a scientific point of view seemed to him a crime. More than this, much that appeared of great importance to others had no weight with one who regarded the mission of science from so high a standpoint and whose refined nature could not fail to despise all ambiguity, empty phrases and affectations in its literature. He considered the great defect in this to be that, whilst each isolated investigation is deemed a personal achievement and quoted as such, important generalisations were regarded as impersonal property. He was by no means a man who could not endure contradiction and was always ready to listen to it when well founded; it was only when the opposition seemed to arise from incapacity and stupidity that he was roused to fierce anger. His standpoint is best described in the following words written to a friend at the end of a keen discussion: "After all, in science as in ordinary life, all hinges upon whether a man accept the general point of view of his opponent; when that is done it is always possible to arrive at some satisfactory conclusion, and I hope this will always be the case with us".

Although the purely intellectual side of his nature outweighed the emotional, he was invariably grateful for the smallest services, and to me he always proved an indulgent, lovable teacher. At the same time he could coldly repel all who were uncongenial to him. He agreed with Goethe "*Sage nur von deinen Feinden, warum willst du gar nicht wissen,*" etc.

As time went on he became more and more dissatisfied with the state of botanical literature. Such dissatisfaction however did not keep him from incessant toil whenever he was well enough and more especially when the sun shone. Like Goethe and many other sensitive natures, he was strongly affected by sunshine or the lack of it. "If you imagine yourself transplanted from Java to Bavaria and that the sun's face has been veiled for the last three weeks by a layer of sail-cloth 100 metres thick, you may form some conception of the vegetation in our garden. The grass and leaves grow as though this were a dairy-farm! Every

one is charmed with our luxuriant vegetation, but there are no signs of blossoms. It is as dark at four o'clock as it would be at the same hour at Christmas, and it has been like this for the last three weeks. I should not complain, liking as I do to take things as they come, but unfortunately I cannot live without sunshine and the lack of it makes me ill."

It was at Würzburg that Sachs first found fit opportunity to develop his talent for teaching. Too often it happens in lecture-rooms that "*man viele sieht, die nicht da sind*" but this did not apply to him. His fascinating, lucid expositions stimulated the students, whilst he knew well how to practically illustrate his subject. He worked incessantly at the materials for demonstrating, drew and painted a number of diagrams, and was constantly adding to his stock of dried plants, alcohol preparations, models and cultures. He considered that all should be in due relation to the subject matter in a scientific lecture as in the acting of a play. In the winter he lectured on general botany (anatomy and physiology) and in the summer on the "Natural History of the Plant World". Besides this he often gave experimental demonstrations in the summer and this necessitated a great deal of work; occasionally he lectured on the history of botany and on the physiological basis of morphology. After 1874 he had a class every term for microscope work.

A great number of botanists worked at one time or another in his laboratory. The first were Gr. Kraus and Millardet (both formerly at Bonn and Freiburg). Among others attracted by him to Würzburg were, Baranetzky, Brefeld, Francis Darwin, Detlefsen, Elfving, W. Gardiner, Godlewski, Goebel, Hansen, Hauptfleish, Klebs, H. Müller-Thurgau, Moll, Noll, Pedersen, Pfeffer, Prantl, Reinke, D. H. Scott, Stahl, Vines, De Vries, Marshall Ward, Weber, Wortmann, and Zimmermann. He insisted upon his pupils being in earnest about science, and he brooked no laziness. Weak natures naturally felt his influence most strongly, but he set a higher value on those from whom he could gain something.

With failing health he withdrew more and more into

himself: "I am beginning to take private pupils again," he writes, "but there is little pleasure in it. When a professor reaches the age of sixty, he ought *eo ipso* to be pensioned off with his full salary; it might be possible to arrange a university that would serve as an almshouse but I would not go into it."

He urged his pupils to make comprehensive studies even as he was constantly striving after wide generalisations. He was a master in the art. We have only to think of his *Experimental Physiology*, his *Text-book* in four editions, his *History of Botany* and his *Lectures on the Physiology of Plants*. Although he wrote with ease, he bestowed great care upon composition, and usually made several rough sketches before the work was done to his satisfaction. In later years he generally dictated, and the *Lectures* were written in this way. The great debt owed by modern botany to his *Text-book* can scarcely be appreciated even yet by the younger generation of botanists.

No entirely satisfactory text-book had appeared since Schleiden's *Outlines*, a book that contained much that was critically suggestive, but, on the other hand, was one-sided and tinged by the author's personal prejudices; nor had the later editions of it been brought up to date with the advance of science. Sachs' book was the first to make Nägeli's and Hofmeister's researches known to the world. It was written in an unusually clear, literary style, and contained all that was best according to "the present state of science" as the title-page says, especially the author's important physiological researches. The letter-press was interspersed with numerous illustrations, chiefly Sachs' own work and not seldom the results of laborious, tedious experiments. These illustrations have been frequently reproduced and, contrary to Sachs' express wish, have become common property. Too often it has been considered quite unnecessary to obtain his consent to the use of the figures, and the appearance of a newer text-book decked out with his own illustrations elicited from him the somewhat bitter though just remark, that a student, using this book, would

surely think that he (Sachs) was employed by the author to illustrate his work. Towards the end of his life the frequent revisions needed for a text-book became a burden to him: he could not make up his mind to a fifth edition and wrote his *Lectures* in a freer style of exposition.

The book, however, that presents the best insight into Sachs' individuality is his *History of Botany*. Nägeli had originally been commissioned to undertake this work, which was to form a part of *The History of Science in Germany* issued by the Royal Academy of Bavaria, but he had soon abandoned the task. It cost Sachs five years' continuous toil. As with all human work it has many defects and omissions, but the lucidity, the profound philosophical bent of Sachs' mind, lend an incomparable charm to the whole. An English translation of this work appeared in 1890.

If I further attempt to briefly characterise Sachs' importance with respect to science it is with a due sense of the difficulties of the case. His activity was so comprehensive, the results of his researches have become through his *Text-book* so largely common property, that it is not easy to briefly set forth what he has done for science. One would have to write a history of botany from 1860 onwards to justly rate his services. But this is by no means the place for such a work, nor do I feel equal to the task. The extracts already given show that he was no one-sided physiologist, and he was fully aware of the fact. "It may surprise you," he writes, "that from my boyhood the mysteries of relationship (systematic botany) have interested me more than those of biology and physiology. I have apparently specialised in the last-named branch of science, because I have always been of the opinion that the ultimate problems of systematic botany can only be solved by physiological methods." His latest treatises most clearly reveal what he meant.

De Bary's remarks with respect to Mohl apply more or less to almost all distinguished investigators (*Bot. Zeitung*, 1872, p. 572). "As regards a number of discoveries for which we are indebted to Mohl, his claims to

priority in them may justly be disputed if this expression be taken to denote the pretension to have first seen or spoken of a thing, . . . the lucid, confident recognition of it is however due to Mohl's observation." But in Sachs' case the remark applies not merely to the observation of facts, to which Mohl confined himself, but to the bringing into prominence the importance of such facts in their relation to the common stock of our knowledge, and to the right ordering of observations in the general building of knowledge; work on which he laid great stress. He writes: "As I read your book I feel anew how much more merit there is in working out a comprehensive subject from reliable sources, and from a higher standpoint, than in constantly supplying fresh contributions, which, however meritorious in themselves, are yet as the scattered stones of the hillside compared to milestones pointing us on our way!"

Sachs is best known and most famous as the founder of the modern physiology of plants, and his physiological works may be next touched upon. "My earliest treatises," he once wrote, "were composed at a time when the physiology of plants was simply non-existent; I myself was entirely self-taught and consequently much of my work was imperfect, especially the manner of exposition." Nevertheless these earlier works are of great importance. Next to be named come his works upon chemical philosophy. The investigations of Ingenhouss, Th. de Saussure, Liebig, Boussingault and others had supplied the foundation upon which, in connection with the results of plant-anatomy, a more exact knowledge of the phenomena of metabolism was to be built up. It was Sachs who first pointed out "that the starch in chlorophyll is not merely a secondary deposit but must be regarded as the product of the assimilating activity (produced by the action of light) of the granular, chlorophyll substance: that it is formed in the chlorophyll out of its original elements and is conducted to the growing buds and to the tissues which store up the reserve material"¹: a brilliant addition to our knowledge, the

¹ *Collected Essays*, p. 335.

fundamental importance of which needs hardly to be demonstrated at the present day.

The formation of starch largely engaged his attention later on. He contrived a simple means of quantitatively estimating starch-assimilation, and by the application of the "iodine test" to leaves or portions of leaves respectively, supplied an extraordinarily simple and instructive method of demonstration.

His services in improving the culture of plants in nutrient solutions are well known. They drew down upon him a violent attack from Knop which deeply wounded him, and not without reason. It is now one of the most elementary experiments in the physiology of plants to rear a plant from germination to seed-bearing by the administration of nutrient salts; but at that time it was maintained that the seed-bearing plants of maize must have been placed in the solution of nutrient salts after they had attained a flourishing condition!

He incidentally discovered the interesting fact that polished marble slabs may be corroded by roots; a fact of some importance for the understanding of the functions of these organs. He began to work upon entirely virgin soil when, at the close of his fiftieth year, he set on foot investigations which brought to light by microscopical tests, and above all by microchemical methods, the movements, chemical changes, and final consumption of the reserve material during the growth of organs. These experiments have also proved of fundamental importance, and he lays stress upon the fact that they served to first lead him to think that the chlorophyll grains are the true organs of assimilation. A bare reference must suffice to the classical treatises on the germination of the date-palm, of grasses, or on inulin, etc.

In later years he ceased to contribute experimentally to our knowledge of metabolism. Other problems had meanwhile claimed his attention. His investigations—the first to be made—into the action of heat claim special notice. The phenomena of freezing had long been in need of investigation, and here also Sachs' work created a clear conception

of the problem and went far towards clearing it up. Even more important were "The Physiological Experiments upon the Dependence of Germination on the Temperature". For by these the law of the "drei Kardinalpunkte" (three cardinal points) was established, and the term "Optimum" introduced for one of them—a name that has been adopted in other departments of science. These experiments were carried out with the simplest appliances, not even in a botanical laboratory, but in his own rooms at Prague. His great manual dexterity and skill in devising simple, but extremely effective, instruments were most useful to him.

The discovery that with sensitive organs there are temporary conditions of rigor due to heat and cold has become an intrinsic part of physiology, whilst the establishment of the fact that not only light, but at the same time a sufficiently high temperature is needed for the formation of chlorophyll in the higher plants, was of great interest.

From amongst the series of researches grouped together in the *Gesammelten Abhandlungen* (*Collected Essays*) under the heading "The Action of Light" I should like shortly to refer to the treatise "Upon the Influence of Daylight on the Production and Development of Different Plant-organs".

The fact that the formation of cells and organs is dependent upon light was submitted in this paper for the first time to a searching investigation; it was shown that the formation of roots was in many cases directly favoured by light; the conclusion was drawn from Wigand's data that with fern prothallia light determines the dorsiventrality, and the phenomena of etiolation, which still present many enigmas, were more closely examined. The investigation into the action of light through the medium of the foliage-leaves upon the formation of flowers, was especially important to Sachs, because it formed the starting-point for his later theory of "Matter and Form". It showed him that plants such as *Tropaeolum*, *Brassica*, etc., continue to produce etiolated stem-parts and leaves in darkness "in sufficient quantity for the production of fresh blooms if this depended only

upon the bulk of the material stored for the purpose and not also upon the particular quality of it," a fact that later led him to form his theory as to the specific matter out of which organs are formed. The formation of blossoms was proved to depend directly or indirectly upon light, inasmuch as by the assimilating activity of the leaves in light, the materials destined to produce flowers are formed. Later research into "The action of the ultra-violet rays upon the production of flowers" seeks to define this phenomenon more closely.

The action of coloured light upon plants in respect to assimilation and to their heliotropic curves, etc., received soon after valuable confirmation. Sachs introduced the simple and convenient method of counting the bubbles given off by water-plants in light, and came to the conclusion (which lately has again been questioned) that the so-called chemical rays have very little to do with the giving off of oxygen.

A keen controversy was aroused by the opinions he formed in consequence of his researches into "The movements of water in plants". But even if his inhibition theory be rejected it must not be forgotten how many valuable facts are due to his activity in this field. The effects produced by the chemical and physical state of the soil upon transpiration, the checking action of salt solutions, low temperatures, etc., were well established; the "Lithium Method" was used for measuring the rate of the transpiration current; and the profound and far-reaching importance of transpiration for the life of most plants was demonstrated.

A further laborious and protracted series of experiments dealt with the phenomena of growth and of movements produced by stimuli. Among the more notable of these are the construction of the first auxanometer, the graphic description of his observations, and the recognition of the grand period of growth. His investigations into the growth of the main and side roots first proved convincingly the factors which condition the regular extension of the root-system in the ground, and established the distribution of growth in roots, as well as the correlation between main and

side roots. A number of isolated observations are also to be found in this exhaustive treatise. Sachs' clear, perspicuous style renders it a pleasure to read any of his essays, even when he is compelled to enter minutely into detail.

The phenomenon of "Hydrotropismus" (the name originated with Sachs) had already been occasionally investigated, but Sachs showed it to be due to irritability, demonstrated its importance and facilitated the examination of it by a simple apparatus. The "Hängende Sieb" (hanging sieve) is now to be found, like the auxanometer and the klinostat, in every botanical laboratory.

The "Tropisms" (Heliotropism, Geotropism, etc.) made large demands upon his time and attention. When under Hofmeister's influence, as regards experimental physiology, he inclined to an external, mechanical conception of these, but abandoned this later. His own words best denote his standpoint: "I too should have nothing to say against the term "Lebenskraft" (vital force) and have indicated as much from time to time in my *History of Botany*, but the word has been spoilt and rendered nugatory by misuse. I say, therefore, to denote my conception of the organic world, that the province of true physiology begins where that of mechanics, physics and chemistry of organisms ends. Indeed, I go farther and maintain that the time will come when in physiology will be found the ultimate basis (what Goethe speaks of as "die Mütter") of all natural sciences. There is no need to say that this vitalistic view did not prevent him from working out with the deepest interest the phenomena of growth-curvatures. He also established the phenomenon known as "after-effects," and contributed many other valuable isolated experiments.

If he attached great importance to theories, he was fully conscious of their transitory nature; and I might mention as an example of this, that in his later years he did not lay so much stress upon his theory of Heliotropism. There will be more to say about this when reference is made to his treatise on orthotropic and plagiotropic organs.

In the meanwhile attention must be directed to the

essays upon the connection between cell-formation and growth, which in my opinion belong to his most brilliant achievements. As a result of Nägeli's researches on the apical cell, numerous botanical works had arisen dealing with the laws of cell-division. It was this tendency, exaggerated until it was justly dubbed "zellfängerei," that led men to neglect plants and organs as a whole for the mere cells, and to take it as granted that growth is determined by the manner and method of cell-division, much as the shape of a building is determined by the way the building-stones are laid one upon another.

Hofmeister's brilliant, though hardly well-grounded, opposition had but little success: only a few botanists took any notice of it. It was Sachs who, in his usual clear manner and by the aid of simple contrivances, first explained the relations between cell-disposition and growth. In his opinion, the latter is the determining factor, the arrangement of cells depending upon growth. This explained why, for instance, cross-sections through cylindrical masses of cells in plants belonging to widely separated groups may present the same appearance of cell-arrangements, as a developing alga or a hair of a dicotyledon. The introduction of the terms "anticlinal" and "periclinal" made a brief, striking bird's-eye view of the matter possible, and facilitated further study of the changes in cell-disposition occurring during growth. A large group of facts was brought together under a common heading; and not only was the way made smooth for further investigations into the causes of the arrangement of cells, but an important point of departure was also made for experiments on the evolution of organs which do not possess an apical cell.

The changes, which had gradually taken place in the cell theory, have led to an entire alteration in its original meaning. This prompted Sachs, who always felt the need of clear and consequently historically correct conceptions, to introduce the definition "*Energid*". In my opinion he thereby rendered good service to science. It was a great satisfaction to him that his achievements found favour with the most eminent histologists (Kupffer for instance), and

this consoled him for the fact that the botanists, now as on other occasions, instead of testing the innovation in its general application, sought only too zealously for instances in which it did not apply. But the time will surely come when it will be deemed absurd to describe a *Caulerpa*, for instance, as a "unicellular" plant, and it fell to Sachs to fit scientific nomenclature to recent advances in knowledge. It was self-evident to him that definitions are only a means towards generalisation and that they have absolutely no validity in themselves.

The essay upon orthotropic and plagiotropic plant-parts takes us into a region that lay nearest to Sachs' heart during the last years of his life, namely, that of physiological or causative morphology. In this treatise he deals with the connection between the structure (in the widest sense of the word) and the direction of the organs. The definitions "orthotropic" and "plagiotropic" were introduced, and referred more particularly to the dorsiventral structures that had long been neglected under the supremacy of the "spiral theory". He does not merely treat of the purely structural conditions, but of the causative relations between orthotropic growth and dorsiventral structure. Sachs would, I believe, have altered later his theoretical conclusions upon plagiotropism; they are based upon ideas which he no longer held, as we may see in the text, to be as thoroughly warranted as formerly. But putting aside these points, about which opinions still differ, we find ideas in this essay that are still working with considerable effect in morphology.

As a morphologist Sachs' activity displayed itself in one direction by some special studies that date from his earlier years, in another by his text-books, and again by his final general essays.

His two treatises on *Collema*¹ and *Crucibulum*, show him at work in the region of cryptogams. It was he who in his *Text-book* defended Schwendener's Lichen theory

¹ In this essay he approached very closely to the later lichen theory, when he said that it looked as if a parasitical fungus had established itself in the nostoc; he believed that the nostoc-heterocysts might develop into a mycellium.

at a time when the cautious De Bary (in his criticisms of the second edition of the *Text-book*) looked askance at it. The Archegoniates are treated in the *Text-book* with special interest, forming part, as they had done, of his own researches. His grouping of thallophytes (in the fourth edition of the *Text-book*), which met with such adverse criticism, has at any rate attained the satisfactory position of being approached again in our own days by many writers.

Throughout his life he cared little for those details that often fill men's lives, and preferred to view matters from a wide and general standpoint. In the first edition of his *Text-book* he had set his face against "idealistic morphology" at a time when it was dominant, and in a paragraph of his *History* that promises to become classical he laid bare the foundations upon which this tendency rested.

Darwinism was another bugbear to him and he intended to attack it vigorously in the *Principles*. "As far as it goes I am delighted to be free from 'the immutability of species' and to be able on good grounds to accept evolution. But it is absolutely uncertain *how* we are to conceive of this latter. Therefore I say that the natural system of classification is only to be explained by Descent, but how *this* is to be explained no one knows. I regard Descent as a fact, like gravitation, about which also we are absolutely in the dark." His whole conception of the world rebelled against "the crude materialism" which he thought he found in Darwinism; "if my *Principles* do not meet with the response I had expected, they have done me good service in showing me that Darwinism as a whole is entirely superfluous for any scheme of the final causes of nature. A superfluous theory has received its sentence."

He sought however to obtain some similar conception of causes by his theory of "organ-forming matter," which caused the external diversity of organs to appear dependent upon their material differences of substance, a view which had its origin in the researches alluded to above on the dependence of bud-formation upon the assimilation activity of the leaves. By this a theoretical basis was gained for

experimental morphology ; deformities, galls, etc., could be referred to definite changes of substance ; and the assumption that stem-forming substances find their way to the point of stem-growth, root-forming to that of the root-system, explained to him most naturally the facts to be seen in reproduction. It is evident that in such a difficult subject one must look for sketches, or general views, rather than theories worked out in detail. But at any rate Sachs' views are more fruitful than Nägeli's "*Idioplasma*," and he made a number of experimental morphological studies on their bases.

He had already arrived at the conception of the continuity of the embryonic substance before the appearance of Weismann's *Germ-plasm*. "That which has maintained itself alive, and has continually reproduced itself since the beginning of organic life upon the earth, moving steadily onward in the eternal change of all structures, in the unvarying alternation of life and death, *that* is the embryonic matter of vegetation and it is this which in certain cases differentiates itself into the two sexes in order again to unite."

He conceived of the multiplicity of plant forms as arising, on the one hand, from the phylogenetic morphological differentiation (this, however, he regarded as an "absolute mystery"), and on the other from the re-action of the common vegetable substance in response to external stimuli (automorphosis and mechanomorphosis). "Adaptation" in Darwin's sense of the expression he considered entirely superfluous, and herein he was in entire agreement with Nägeli. He expressed his views in a powerful manner in his last writings—the physiological "Notices"¹ published in "*Flora*". The manuscript found after his death, entitled "The Principles of Vegetable Formation," has been handed over to Prof. Noll for publication.

This slight sketch can give but an inadequate idea of Sachs' lifework with its abundant results as regards science ; indeed I can but liken what I have written to a man striking

¹ These will shortly appear as a separate publication.

one by one a few strings of an instrument that has answered to the touch of some great musician.

One may well say with the Psalmist in speaking of his days :—

Yet is their strength but labour and sorrow.

Nevertheless his life has borne rich fruit ; his name is for ever bound up with the history of botany. He has enriched this science by the discovery of new and important facts and conceptions and by his unrivalled power of clear definition. In the nature of things it is impossible that all his theories should retain acceptance, but they have all profoundly influenced his contemporaries. There is no doubt that in any other calling Sachs would have risen to the first rank ; eccentricities and narrow “specialising” were alike repugnant to him. In the last years of his life he applied himself eagerly to palaeontological and zoological studies ; “I must be learning, always learning,” he wrote in a letter. In spite of his incessant labours he was one of the few men of the present day who possess the gift of letter-writing and withal a spirited style, clear and trenchant.

And yet these letters, written during the last fifteen years of his life, form one long report of illness.

At last Death, who in the latter years had often drawn very near, took him gently by the hand and led him to his final rest.

K. GOEBEL.

ASSOCIATION AND DISSOCIATION.

IT is now almost universally granted that the molecular weights of volatile compounds can be directly determined by measuring their vapour densities. It is only by acceptance of Avogadro's law that chemists bring into reconciliation with the atomic theory the known facts with reference to combining proportions by weight and by volume, and physicists deduce the same law in simple manner from the kinetic theory of gases.

Until recently, direct determinations of molecular weights were only possible in the case of volatile compounds, and our knowledge of molecular weights was therefore in the main derived from the study of vapour densities. And even now this method is the only one that can be regarded as of general application. For although the researches of Raoult and the generalisation of Van't Hoff with reference to dilute solutions, lead to the view that the dissolved substance in such solutions is in a condition comparable with that of the same substance when vaporised, the consequent application of Avogadro's law to dilute solutions has not been invariably attended with success. Exceptions of a very baffling character are too frequently noticed which still await satisfactory explanation; as, for example, that the molecular weights of certain metals, determined by Ramsay from the reduction which they effect in the vapour pressure of mercury, are found to be only about half their usually accepted atomic weights. While this is the case we are therefore compelled to fall back on vapour densities as forming the final criteria in molecular weight determinations.

But as long as our knowledge of molecular weights is mainly confined to what may be learnt from the study of vapour densities it is obvious that it can only apply in all strictness to matter in the gaseous state. Of the molecules of liquids and solids we could learn little in this way, unless it were possible to show that a change

of state takes place without any change in molecular complexity, which is, in many cases at any rate, highly improbable. In fact if the vapour density of a given substance is known, it would be necessary to ascertain whether and to what extent molecular changes accompany liquefaction and solidification, before the molecular weight of the substance as liquid or as solid would be known.

A large number of attempts have been made from different sides to gain an insight into the molecular conditions of liquids and solids, and in the case of liquids the problem has now been brought within a measurable distance of a successful solution. For methods have been devised which serve to show whether the molecules of a liquid are more complex than those of its vapour, and which enable the degree of complexity to be determined approximately in certain cases.

The most successful of these methods is undoubtedly that due to Ramsay and Shields. This method is based on the fact that the molecular surface energies of different liquids are found to be the same at comparable temperatures. As the molecular surface energy is the product of the surface tension, and of the surface which contains unit number of molecules, in practice the method is reduced to the determination of the surface tension of the liquid at different temperatures, for which only simple apparatus is necessary and no special skill on the part of the experimenter is required.

The investigation of the molecular surface energy of different liquids has clearly shown that liquids must be divided into two classes. A large number of liquids turn out to have the same molecular weights as their vapours, and these liquids have therefore been termed normal or monomolecular. On the other hand certain liquids are found to have molecular weights which are greater than those of their vapours, and such liquids have been termed associating. With liquids of this last class, if M is the molecular weight deduced from the vapour density, the molecular weight of the liquid will be xM , where x is called the factor of association. As far as has been at present ascer-

tained, the hydrocarbons and their halogen derivatives, the ethers, the ethereal salts, and acid chlorides and anhydrides are monomolecular liquids. Among associating liquids one must in the first rank place those compounds which contain the hydroxyl group, as the presence of this group appears to have the greatest influence in inducing association. Thus water is found to occupy quite an exceptional position among liquids, with a factor of association that is probably higher than that of any other. The alcohols, and those acids which contain the hydroxyl group, are also associating liquids. The presence of certain other groups, among which may perhaps be reckoned the amidogen and nitro-groups, induces association, but probably in lesser degree than does the hydroxyl group.

In addition to the surface tension method, there are several other means of judging whether a liquid is monomolecular or associating. None of these are, however, so exact in principle as the method described, and the degree of association can be only roughly gauged by these processes. The chief methods are those based on an examination of the liquid when it is undergoing a change of state, either to gas or to solid, for it is fairly evident that at these points the influence of association should be rendered apparent in the most marked manner.

Several methods of this kind have been indicated by Guye, based chiefly on the behaviour of liquids either at or near their critical points. Exceptions to Trouton's law, that the molecular latent heat of vaporisation divided by the boiling-point on the absolute scale is approximately constant for all liquids, occur, as Linebarger has shown, only with associating liquids. The molecular volumes of monomolecular liquids are approximately proportional to their boiling-points on the absolute scale, the value of T/mv being about 3.5; but in the case of associating liquids this rule no longer holds and T/mv has a much higher value (in the case of water 19.85). Combining this last result with Trouton's law, it appears that the product of the latent heat of vaporisation and the density at the boiling-point (the latent heat of vaporisation of unit volume of the

liquid) is, in the case of monomolecular liquids, about 75, and has generally a higher value for associating liquids. The latent heat of fusion affords another indication of association in liquids. The latent heat of fusion of *unit volume* divided by the temperature of fusion on the absolute scale is approximately constant and equal to 0.1 for monomolecular liquids. Associating liquids give a higher value and one that is not far removed from 0.1x.

Liquids then are of two kinds, monomolecular and associating. A full recognition of this fact is of the highest importance from the physico-chemical point of view, as it serves to throw light on many of the vexed questions of physical chemistry. And in the first place it is necessary to clearly understand the character of the difference between the two classes of liquids.

Monomolecular liquids are liquids the molecular weights of which are identical with those of their vapours when in the state of perfect gas. On the liquefaction of the vapour no change takes place in molecular composition, and the liquid on being heated or cooled also suffers no change in molecular composition. Such a liquid is therefore from this point of view a perfectly stable system, the composition of which is not affected by outside influence.

Associating liquids are liquids the molecular weights of which are not identical with those of their vapours when in the state of perfect gas. In this case on the liquefaction of the vapour a change occurs in molecular composition, and the molecules of the liquid produced are more complex than those of the vapour. But the change is not complete on liquefaction, for the researches of Ramsay and Shields have clearly shown that the factor of association is not a constant in the case of any one associating liquid, and that it alters with the temperature. As the temperature falls the factor of association increases and the liquid becomes more and more complex in composition; with a rise in temperature this change is reversed. From this point of view then, an associating liquid is not a stable but a labile system, the composition of which is subject to alteration with change of temperature and perhaps other outside conditions. It is

this labile character of associating liquids which forms the most important difference between liquids of this class and those of the stable monomolecular order.

Now hitherto it has been the general custom to compare all liquids one with another in perfectly indiscriminate fashion, in the hope of establishing general relationships among liquids that might rank with those already established for gases. This method has been attended with a certain degree of success, and regularities of a striking order have been observed among the molecular volumes, the boiling-points, the molecular refractions, and other properties of liquids. But exceptions of a very puzzling order frequently arise along with the regularities observed, exceptions for which in a large number of cases it has, up to the present, been impossible to account satisfactorily. It can now hardly be doubted that one great cause of such exceptions is to be found in the fact that monomolecular and associating liquids have been indiscriminately compared one with another, when strictly speaking they are not truly comparable.

Investigation shows that such regularities as have hitherto been observed appear in most marked manner when monomolecular liquids alone are compared one with another. The exceptions arise, as might be expected, when associating liquids are included in the list, for associating liquids are neither comparable with monomolecular liquids, nor are they directly comparable with one another.

In this connection it is important to call special attention to the case of water. Water occupies almost a unique position among liquids on account of its high factor of association, which is greater than that of nearly all other associating liquids. In all comparisons of liquids one with another, water should therefore strictly speaking be given a place apart and be regarded as exceptional. Unfortunately up to the present, for practical reasons, it has been customary to select water as the typical liquid compound, and to employ it always as the standard of comparison. The choice is an unlucky one for the reasons stated, and one that should be as far as possible abandoned.

Among those properties of liquids on which association is likely to have a marked influence, and to which a peculiar interest attaches, must be ranked their behaviour as solvents. That the solvent properties of a liquid are influenced by its degree of association, is evidenced by the fact, that, as a general rule, associating liquids more readily dissolve associating liquids, and monomolecular liquids more readily dissolve monomolecular liquids, than liquids of the one class dissolve those of the other. Compounds containing hydroxyl are, generally speaking, insoluble in the hydrocarbons, in ether or in carbon bisulphide, but soluble in water and alcohol. The higher hydrocarbons, the fats and waxes, are insoluble in water and almost insoluble in alcohol, but dissolve in other hydrocarbons, in ether and in carbon bisulphide.

Not only is the behaviour of liquids as solvents influenced by their association, but in the properties of the solutions formed the influence of association can still be traced. It has been shown by Linebarger that when monomolecular liquids are mixed one with another, the densities of the mixtures are very nearly equal to those calculated from the densities of their components, no allowance being made for contraction or expansion. Nothing of the kind holds for mixtures of associating and monomolecular liquids, or for mixtures of associating liquids with one another, for here so great a contraction or expansion occurs that the law of mixtures does not apply.

Reference has been previously made to Van't Hoff's application of the gaseous laws to dilute solutions. As is well known, according to Van't Hoff, the osmotic pressure of a dissolved substance is identical with the pressure which the same substance would have in the gaseous state, if the solvent could be completely removed so as to leave the dissolved substance in that state. Solutions therefore which contain at the same temperature and in the same volumes equal numbers of dissolved molecules should have the same osmotic pressures. In the majority of cases this is found to be so, but a large number of exceptions have been observed, the osmotic pressure being

in some cases smaller and in others greater than it should be according to the above rule.

There is little difficulty in explaining those exceptions to the osmotic pressure rule which occur when the osmotic pressure found is smaller than the theoretical. Such cases usually arise when associating compounds are dissolved in monomolecular liquids, and it is now generally admitted that in these cases the dissolved compound is still in an associated condition, so that the number of molecules present in the solution is less than that calculated on the assumption that the dissolved compound is monomolecular. But if a smaller number than the calculated number of molecules is present, the observed osmotic pressure will also be smaller than that calculated, and this is what is actually found to be the case.

A greater osmotic pressure than the theoretical is met with in the case of aqueous solutions of metallic salts, and also of solutions of the same salts in formic acid and some other associating liquids. In these cases the explanation first advanced by Arrhenius is commonly accepted, and the dissolved compound is believed to be more or less in a state of electrolytic dissociation. Assuming that the molecules of the dissolved salt are dissociated into their ions, and that each ion has the same effect in establishing osmotic pressure that a separate molecule has, the observed osmotic pressure must of course be greater than that calculated on the assumption that the dissolved compound has gone into solution without any change in molecular composition.

But the abnormal osmotic pressures of electrolytes can be explained without recourse to the theory of electrolytic dissociation, and by reference solely to the facts regarding liquids which have been just considered. When a solution is formed by dissolving a given compound in a given solvent, four possible cases are presented :—

- (1) The solvent and the dissolved compound may be both monomolecular.
- (2) The solvent and the dissolved compound may be both associating.

- (3) The solvent may be monomolecular and the dissolved compound may be associating.
- (4) The solvent may be associating and the dissolved compound may be monomolecular.

In the first and second cases experience shows that the osmotic pressure of the resulting solution is usually normal, and agrees with that calculated by the Van't Hoff rule. The third case has been already considered, and it has been shown that a lower osmotic pressure than the theoretical should be, and actually is, observed. For the fourth case it is impossible at present to bring forward recognised instances, but it is highly probable that electrolytes belong to this class of solutions.

In the first place evidence is gradually accumulating that electrolytes are only formed when the solvent is associating. Solvents which, with dissolved metallic salts, yield solutions having a high electrical conductivity, usually contain the hydroxyl group, and are associating. And other associating solvents which do not contain the hydroxyl group, as, for example, acetone, propionitrile and nitroethane, have been lately shown by Miss Aston and P. Dutoit to yield with metallic salts solutions of high conductivity. On the other hand, Kablukoff found that when a monomolecular solvent is taken, the resulting solution has practically no conductivity. That the dissolved salt is either monomolecular or approaches to this condition cannot be shown directly, as no satisfactory examination of metallic salts in the liquid state can be carried out. But ethereal salts as a class are monomolecular compounds, and analogy would therefore point to this being true of the metallic salts. It will also be noticed that those compounds which dissolve in water and other associating solvents without formation of electrolytes belong to the class of associating compounds; as, for example, alcohol, glycerol, and cane sugar.

Now it has been pointed out that when, as in the instances last quoted, the solvent and the dissolved substance are both associating (Case II.) normal osmotic pressures are obtained, although when the solvent is mono-

molecular and the dissolved substance is associating (Case III.) lower values than the normal are obtained. This is not what one would at first expect, for there seems no immediate reason why, in an associating solvent, a dissolved substance which is also associating should give values which are different to those obtained when the solvent is monomolecular. It is evident in fact that the difference in the solvent exercises the determining influence in these cases and not the character of the dissolved substance, as otherwise an associating compound would in *all* solvents give lower values for the osmotic pressure than the normal. The difficulty has been usually explained by assuming that an associating solvent brings about a dissociation in any dissolved compound of an associating type, and reduces it to the monomolecular condition. The dissociation here spoken of is not electrolytic dissociation, but a breaking down of complex molecules into simple ones.

It is difficult to admit the universal dissociating influence thus assigned to associating solvents, without any explanation of the why and wherefore of its existence. For according to this view every compound, whatever its character, when dissolved in any associating solvent, must be regarded as undergoing dissociation. If the compound is associating, the action merely proceeds as far as the breaking up of the complex molecules into simple ones, and the dissociation is therefore of the ordinary kind. But if the compound is monomolecular, the action is of a more complex character, for the molecules themselves are broken up, and so-called electrolytic dissociation occurs.

A far simpler view of the matter is obtained if it is allowed that the abnormal behaviour of solutions containing an associating solvent is not due to any change in the dissolved compound, but due solely to the character of the solvent itself, and its influence on the osmotic pressure. It has been already pointed out that monomolecular and associating liquids are not directly comparable one with another. Admitting the validity of the osmotic pressure formulæ when the solvent is a monomolecular one, it does not therefore follow that these formulæ will necessarily

apply when the solvent is associating. The probability is that some suitable modification is required and the introduction of a factor due to the associating character of the solvent.

It can be shown that, if the influence which the association of the solvent is here supposed to exercise on the osmotic pressure be admitted, the osmotic pressure of a solution containing an associating solvent should always be greater than that of a solution of similar concentration with a monomolecular solvent. Solutions of Class II. might therefore be expected to exhibit osmotic pressures that would approximate to the normal calculated values, and solutions of Class IV. would have osmotic pressures greater than the normal. In fact solutions of Class IV. would possess the behaviour met with among electrolytes, and as shown above electrolytes do in all probability belong to this class of solutions.

The supporters of the theory of electrolytic dissociation deny that the character of the solvent can have any direct influence on the osmotic pressure of a solution. The thermo-dynamical treatment of the subject leads to this conclusion, and any contrary view argues a flaw in the train of reasoning by means of which this result has been obtained. The difficulties that arise in applying thermodynamics to chemical investigations have been well illustrated by Fitzgerald in his Helmholtz Memorial Lecture. And a difficulty which has been overlooked occurs in dealing with dilute solutions. A monomolecular solvent does not change in composition with rise or fall of temperature, but any change in temperature at once alters the composition of an associating solvent. As temperature enters as one of the factors into thermo-dynamical equations, in the treatment of dilute solutions some distinction should be drawn between those which contain monomolecular and those which contain associating solvents. But up to the present no such distinction has been made and all liquids have been treated as exactly alike.

In opposing to the theory of electrolytic dissociation the view that association of the solvent influences the osmotic

pressure of solutions, and that the exceptional behaviour of electrolytes is due to this cause, it must be urged that the results of the thermodynamical treatment of this question have not up to the present been conclusive. A full recognition is necessary of the fact that in accordance with their differences in composition it is necessary to sharply divide liquids into two different classes. The treatment applied to liquids of the one class cannot be indiscriminately extended to those of the other, for if this is done the results obtained will be of an altogether misleading character.

HOLLAND CROMPTON.

RECENT EXPERIMENTS IN HYBRIDISATION.

IT has long been recognised that the facts of hybridisation have an important bearing on many subjects of biological interest. The question of the infertility of crosses was very carefully investigated by Darwin, and since his time has been more than once discussed afresh ; while the structural and other characters presented by hybrid offspring are of undoubted weight in reference to the problem of heredity.

For some years past Dr. M. Standfuss, of Zürich, has been experimenting on a large scale in the production of insect hybrids. The results of these experiments, some of which confirm previous views, while others appear to suggest conclusions not hitherto reached, have now been collected by their author in his *Handbuch der paläarktischen Gross-Schmetterlinge*, published at Jena in 1896. Many of the data thus made available derive especial value from the fact that they are expressed numerically, and the whole series forms a noteworthy contribution to our knowledge of the subject. It is proposed to give here a short account of these experiments, with their results, for the benefit of those readers to whom the original records may be difficult of access.¹

The author prefaces his account with a list of fertile and infertile pairings between different species of macrolepidoptera that have been observed by himself and others, both under natural conditions and in captivity. Details are first given of twenty-four cases of pairing between *Bombyx neustria* L. ♂ and *B. franconica* Esp. ♀. The results showed every transition between complete absence of issue and the deposition of eggs normal in numbers and fertility. Failure was in some instances plainly due to inadaptability of the genital apparatus. On this series of pairings Stand-

¹ Several of Dr. Standfuss's specimens have lately been exhibited in London, at the rooms of the Royal Society, Burlington House and at the Natural History Museum, South Kensington.

fuss remarks that the absence of uniformity in the results shows that several trials should be made before any given cross is pronounced infertile.

Five crosses are mentioned from which only male offspring are known to have resulted; these are *Deilephila porcellus* L. ♂ and *D. elpenor* L. ♀; *Smerinthus austanti* Stgr. ♂ and *S. atlanticus* L. ♀; *Fumea nitidella* Hof. ♂ and *F. affinis* Rutt. ♀, with the reciprocal cross *F. affinis* ♂ and *F. nitidella* ♀; *Bombyx neustria* L. ♂ and *B. franconica* Esp. ♀.

In the following five cases only female offspring have been observed, and these contained no eggs capable of development: *Bombyx neustria* L. ♂ and *B. castrensis* var. *veneta* Stdts. ♀; *B. franconica* Esp. ♂ and *B. castrensis* var. *veneta* ♀; *B. quercus* L. ♂ and *B. trifolii* Esp. ♀; *Saturnia pyri* Schiff ♂ and *S. pavonia* L. ♀; *Drepana curvatula* Bkh. ♂ and *D. falcataria* L. ♀.

In the following seven the males predominate and the females are again sterile: *Deilephila euphorbiæ* L. ♂ and *D. vespertilio* Esp. ♀; *D. hippophaes* Esp. ♂ and *D. vespertilio* Esp. ♀; *Smerinthus ocellatus* L. ♂ and *S. populi* L. ♀; *Saturnia spini* Schiff ♂ and *S. pyri* Schiff ♀; *S. spini* Schiff ♂ and *S. pavonia* L. ♀; *Harpyia vinula* L. ♂ and *H. erminea* Esp. ♀; *Notodonta dromedaria* L. ♂ and *N. torva* Hb. ♀.

The author points out that no fertile female offspring having been observed in any of the above seventeen crosses, none of these hybrids, so far as our present knowledge goes, could continue their race. It is unfortunate that the actual numbers of the broods in question are not given; in some instances, as in that of *B. neustria* ♂ and *B. franconica* ♀ they are presumably large.

In three more crossings the offspring consisted of both sexes in normal proportions; the females, however, contained at best but few eggs, and these abnormal in structure. The crossings were of *Smerinthus populi* L. ♂ and *S. ocellatus* L. ♀; *Saturnia pavonia* L. ♂ and *S. pyri* Schiff ♀; *S. pavonia* L. ♂ and *S. spini* Schiff ♀.

Finally, in two more the sex-distribution appears again

to be as usual, and the females contain ova normal in appearance. Their fertility, however, has not been proved, and Standfuss thinks he has reason to doubt it. These are *Zygæna trifolii* Esp. ♂ and *Z. filipendulæ* L. ♀; *Biston hirtarius* Cl. ♂ and *B. pomonarius* Hb. ♀.

In no single instance, according to Standfuss, has the female of any true hybrid among lepidoptera been shown experimentally to be fertile. Haeckel's counterstatement with regard to the genera *Saturnia* and *Zygæna* is probably erroneous.¹

The most complete series of Standfuss's experiments in hybridisation is one carried on for over ten years with three species of *Saturnia*, viz., *S. pavonia* L., *S. spini* Schiff, and *S. pyri* Schiff. The results of these experiments are recorded and compared by their author with great minuteness, and excellent figures are given by him of several of the resulting forms in their immature and final stages. A brief outline of these records, omitting details, will here be attempted.

1. *Saturnia pavonia* ♂ and *S. spini* ♀. In this hybrid, called by Standfuss *S. bornemanni*, the eggs first laid were regularly deposited, and were fertile to the extent of from 60 to 80 per cent. The larvæ in all five stages bore a much closer resemblance to those of *S. spini* than to those of *S. pavonia*, though this became less pronounced after the second stage was passed. The cocoon and pupa were both intermediate in structure, but the perfect insect was nearer to *S. spini* than to *S. pavonia*. This applies to both sexes, but is more easily seen in the male, the males of the two parent species differing (as is usual) more than the females.

2. The reciprocal cross (*S. spini* ♂ and *S. pavonia* ♀; = *S. hybrida* Ochs.) has not been produced in captivity,

¹ Standfuss does not notice the case cited from Quatrefages by Wallace (*Darwinism*, 1889, p. 163), of fertility *inter se* in the hybrids between *Bombyx cynthia* and *B. arrindia*. Fletcher also has obtained fertile hybrids of both sexes between *Zygæna loniceræ* Esp. and *Z. trifolii* (*Proc. Ent. Soc. Lond.*, 1893, p. ix.). On the other hand, the hybrids of *Z. loniceræ* and *Z. filipendulæ* proved infertile (*Ibid.*, 1891, p. ix.), and the hybrid progeny of various species of *Platysamia* and *Actias* were found by Miss Morton to be sterile in both sexes (*Ibid.*, 1895, p. xxxiv.).

but is repeatedly found in the wild state, generally as a larva. The question naturally suggests itself, how the parentage of this hybrid is known. Standfuss replies that the larva, though intermediate in character, shows constant differences from that of *S. bornemanni*, and that the conditions in nature for the pairing of *S. spini* ♂ and *S. pavonia* ♀ are favourable, whereas those for the converse cross are difficult or impossible. The males of both species emerge before the females, and in regions where both occur *S. pavonia* is out first. Hence, though the females of *S. pavonia* are out with the males of *S. spini*, the converse does not take place. It must be allowed that it would be well to test this conclusion as to the parentage of *S. hybrida* by experiment. The larva of *S. hybrida* resembles *S. pavonia* somewhat more than does that of *S. bornemanni*; it possesses, however, the greasy polish of *S. spini* to a greater extent than the latter. The cocoon and pupa both show a nearer approach to *S. spini* than do those of *S. bornemanni*, and the same applies to the perfect insect; in fact the resemblance of both sexes to the male parent is remarkably close.

3. *S. pavonia* ♂ and *S. pyri* ♀. This crossing produced hybrids which fell into two classes, a dark form called by Standfuss *S. daubii*, and a paler form to which he gives the name *S. emiliæ*. The latter is by far the commoner. As in former cases, if the laying is much deferred after pairing, the eggs are apt to prove infertile. The larva in its early stages is very like that of *S. pyri*; it becomes more and more like that of *S. pavonia*, and finally bears a close resemblance to the latter species. The cocoon is intermediate; the pupa is nearer to *S. pavonia* than to *S. pyri*. The perfect insect, except for a reduction in the sexual disparity of size, is more like an enlarged *S. pavonia* than a diminished *S. pyri*. In a majority of specimens some of the veins were forked terminally at a greater or less distance from the margin of the wings.

The males of this hybrid paired readily with the females of *S. pavonia*; they were also attracted by the female hybrids, though in a less degree. The latter were, as already

stated, infertile, the oviducts containing no ripened eggs. Pairing also took place, though with much less readiness than in either of the preceding cases, between the hybrid males and some females of *S. pyri*. The number of fertile eggs produced in the latter case was far smaller than in the cross with *S. pavonia*.

4. *S. bornemanni* ♂ and *S. pavonia* ♀ (*S. bornemanni* is the cross-product of *S. pavonia* ♂ and *S. spini* ♀). The larvæ at first closely resembled those of *S. pavonia*, but in subsequent stages the influence of *S. spini* began to assert itself. They presented a very variable appearance on reaching the fourth stage, which period unfortunately they did not survive. Fresh batches, however, have since been raised to maturity, though no record of their history has yet been published.¹

5. *S. emiliæ* ♂ and *S. pavonia* ♀ (*S. emiliæ* is a cross-product of *S. pavonia* ♂ and *S. pyri* ♀). The larvæ of this hybrid (named *S. standfussi* by Wiskott) are again very variable. Their general appearance is that of a large *S. pavonia*. The cocoon and pupa are also near the same parent species. The perfect insect, like the larva, is variable; it always, however, shows much resemblance to *S. pavonia*, and the sexes are dissimilar as in that species. The margins of the wings are apt to be scalloped. The oviducts of the only female that emerged contained mature eggs, but only about twenty, or one-tenth of the normal number in *S. pavonia* or *S. pyri*. It is possible that these hybrids may be fertile *inter se*.

6. *S. emiliæ* ♂ and *S. pyri* ♀ (*S. risii* Stdfs.). This pairing was only obtained with great difficulty, and in four cases out of nine the eggs, though laid in normal numbers, did not hatch. In the remaining five broods only one per cent. produced caterpillars. These at first closely resembled *S. pyri*, and in the second stage still showed more likeness to *S. pyri* than to the male parent form. In the third stage the *S. pyri* characters began to be lost, and in the fourth

¹ Specimens of this hybrid have been reared by the present writer from eggs kindly sent him by Dr. Standfuss in 1895. Other individuals raised from the same batch of eggs are in the Hope Collection at Oxford.

those of *S. pyri* and *S. pavonia* were fairly balanced. After changing its last skin the larva resembles a large *S. pavonia*, though its parentage is three parts *S. pyri*. The perfect insects, of which only six were reared, were in many respects remarkable. Unlike *S. pyri* they were sexually dimorphic, the females differing little from those of *S. emiliæ*, while the males showed a nearer approach to *S. pyri*. The females were not dissected, but were probably sterile. Of the six specimens, one was hermaphrodite and three others showed a tendency in that direction. These four were the produce of three separate females out of the five that laid living ova. Standfuss draws attention to the fact that the normal occurrence of hermaphrodites in lepidoptera is given by Speyer as about 1 in 30,000. The latter number he considers rather too low than too high. There was no hermaphroditism in any known member of the families of the progenitors of these hybrids; and its appearance in this proportion must be, he thinks, a consequence of their exceptional origin. On the other hand, he has bred more than 1000 genuine hybrids without one such case occurring,¹ nor was any such tendency shown by a single one of the sixteen specimens of *S. standfussi*—a form whose origin is analogous to that of the present cross-product.

In the well-marked hermaphrodite mentioned above, the distribution of sexual characters is remarkable. The shape of the fore wings is rather female than male, the colouring on the upper side of both is male; on the under side, the right is mostly male and the left female. In both hind wings the upper surface has the costal portion male, the remainder female; the right, which is about one-fifth larger than the left, has the female area more extensive. The under surface of the right or larger hind wing is female, of the left mostly male. The right antenna is male in form; the left partly male and partly female. The external genital organs on the right side are of a malformed male type; on the left side absent.

¹ Barrett (*Lepidoptera of the British Islands*, vol., ii., 1895, pp. 10, 11) gives some details of the cross between *Smerinthus ocellatus* and *S. populi*. These are said to be "often gynandrous".

The want of vigour in this cross is shown by the fact that out of nine pairings, each resulting in an average of 200 eggs, only ten larvæ were produced; and of these only six, as we have seen, attained the perfect condition.

7. *S. pavonia* ♂ and *Actias luna* L. ♀. Nine apparently normal pairings took place, and over 1000 eggs were laid; but none of these hatched.

8. *S. pavonia* ♂ and *A. isabellæ* Graells ♀. As a result of this crossing, ninety-eight eggs were laid, seven of which hatched. The caterpillars however did not long survive their first change of skin.

9. *S. bornemanni* ♂ and *S. pyri* ♀. As *S. bornemanni* is the cross-product of *S. pavonia* ♂ and *S. spini* ♀, this hybrid is descended from all three species.¹ Only one pairing was obtained, and 92 per cent. of the eggs hatched. Some of the resulting perfect insects were lately shown in London with the other exhibits of Dr. Standfuss, who promises to publish further details of their history.

Relative phylogenetic age of the three European species of Saturnia. Before stating the general conclusions at which he has arrived as a result of the above series of experiments, the author proceeds to discuss the question of the relative phylogenetic age of the three species *Saturnia spini*, *S. pavonia* and *S. pyri*. Of these he considers *S. spini* to be the oldest form and *S. pyri* the most recent. His main reasons are briefly as follows:—

(1) The larva of *S. spini* maintains its original black colour throughout its life. *S. pavonia* loses this sometimes in the third stage, always in the fourth and fifth. *S. pyri* abandons it almost completely in the third stage and onwards. The succeeding green colour, which is no doubt adaptive, is acquired by *S. pyri* at an earlier stage and more completely than by *S. pavonia*.

(2) In the larva of *S. spini* the tubercles are not very prominent even in the adult, and the knobs at their summit are not distinctively coloured until the last stage. In *S.*

¹ Mr. A. G. Butler informs me that an analogous triple cross has been obtained between a goldfinch and the hybrid issue of the English and Japanese greenfinch.

pavonia the tubercles are conspicuous and the knobs acquire an appearance distinct from the general surface of the body in the third or fourth stage. In *S. pyri* on emergence from the egg the knobs are already coloured and indications exist of the extreme prominence of the tubercles.

(3) The tactile bristles are least developed in *S. spini*, most in *S. pyri*.

(4) The cocoon of *S. spini* is simpler than that of *S. pavonia*. That of *S. pyri* is the best defended of the three.

(5) *S. spini* is almost sexually monomorphic, though the male is somewhat the smaller. The female is very sluggish, and the antennæ of the male are pectinate to a high degree. In *S. pavonia* there is well-marked sexual dimorphism, as regards both size and colouring. The female resembles that of *S. spini* in aspect; it is less sluggish, though not very active. The antennæ of the male are less strongly pectinate. *S. pyri* again is sexually monomorphic; the female is a tolerably good flier, and the antennæ in the male are less strongly pectinate than in many other species of *Saturnia*.

Judging from the perfect insects alone, we must conclude, according to Standfuss, that the less specialised *S. spini* is older than *S. pavonia*. We should, however, find it difficult to determine whether *S. pyri* arose before or after the divergence of these two. The larval and pupal stages enable us to answer the question. The three species form a progressive series in protective adaptation, *S. spini* always taking the lowest and *S. pyri* the highest step in the scale. It must therefore be concluded, on the whole evidence, that *S. spini* is phylogenetically the oldest, and *S. pyri* the youngest of the three forms.

General conclusions with reference to hybridisation between distinct species. As the author elsewhere expresses it, the crossing of two distinct species gives rise to a "Zwischenform" but not to a "Mittelform". The Mittelform may, however, exist as a temporary stage in larval growth. This depends on the following principles, which Standfuss considers to be warranted by the above experiments with species of *Saturnia* :—

1. The freshly-hatched larva closely resembles the female parent.

2. With the process of growth a resemblance to the male parent gradually increases.

3. The final extent of approximation towards the male parent depends on the relative phylogenetic age of the two species; the older being able to transmit its properties, whether of structure or habit, better than the younger.

Thus the crossing of *S. pavonia* ♂ with the phylogenetically younger *S. pyri* ♀ gives rise to a larva in which at first the maternal and afterwards the paternal characters predominate.¹ The resulting perfect insect is by more than two-thirds of its external appearance *S. pavonia*, and by less than one-third *S. pyri*. Its habits and functions correspond with its external aspect. It prefers to fly by day, like *S. pavonia* ♂, and pairs easily with the female of that species, from 43 to 62 per cent. of the eggs being fertile. On the other hand, it does not pair readily with *S. pyri*, and the resulting eggs on an average of nine cases gave only one larva from 180.

Similarly *S. pavonia* ♂, when paired with the phylogenetically older *S. spini* ♀, gives a form of which in the perfect state about two-thirds of the external aspect belong to the type of *S. spini*. The male flies by night. After crossing with *S. pavonia* ♀ the resulting eggs were only fertile to the extent of 16 to 22 per cent.; while the crossing with *S. spini* ♀, though not easily brought about in consequence of their diverse times of appearance, yielded eggs of which from 94 to 98 per cent. were fertile.

Thus the male *S. pavonia* is able to influence the issue of the relatively gigantic *S. pyri* ♀ much more than that of *S. spini* ♀.

Again, the issue of *S. spini* ♂ and *S. pavonia* ♀ is much nearer *S. spini* than is that of *S. pavonia* ♂ and *S. spini* ♀. Hence,

4. In reciprocal pairing the male is able to transmit the characters of the species in a higher degree than the female.

¹ Barrett (*loc. cit.*, p. 11) says, quoting Porritt, that the larvæ of the cross between *Smerinthus ocellatus* and *S. populi*, "though like those of *S. populi* when young, appear to become intermediate, or even to resemble those of *S. ocellatus* when full grown".

The above cases show that the former influence, *viz.*, that of the older-established species, is the more effective of the two. The sexual prepotency of the male *S. pavonia* counts for less than the specific prepotency of the female *S. spini*. The highest effect of course is produced when the two influences concur, as in the hybrid of *S. spini* ♂ and *S. pavonia* ♀.

A further result of the experiments is that while no female hybrid was proved to be fertile, there are undoubted cases of fertility in male hybrids. This has been shown by crossing with the females of both parent species, and in one case with a female of a third species (*S. bornemanni* ♂ and *S. pyri* ♀).

Hence hybridisation is not necessarily a merely transitory phenomenon. There seems to be no reason why a male hybrid should not propagate by "back-crossing" under natural conditions; and since the product of this kind of crossing is not found to show a complete reversion to the type of the female parent, it is possible that the existence of various intermediate forms in such genera as *Melitæa*, *Zygæna* and *Agrotis* may be accounted for in this manner. Cases of simple pairing between distinct species of the two former genera have been observed by the author in nature.

The above conclusions with the facts on which they rest, though much condensed from the original, have been stated as far as possible in Dr. Standfuss's own words. A few brief comments seem called for.

(1) The rule laid down by Standfuss as to the prepotency of the phylogenetically older species is probably another expression of the fact so clearly established by Darwin¹ that hybridisation frequently leads to reversion. It is significant that Standfuss considers the hybrid form *S. emiliæ* as partly reproducing an ancestral stage in the history of *S. pavonia* rather than the form of that species at present existing.

(2) It is well known that when plant hybrids are crossed with one of the parent species, considerable variability may

¹ *Animals and Plants under Domestication*, 1868, vol. ii., p. 254, etc.

occur in the offspring (Focke, quoted by Weismann). The facts above recorded as to the results of back-crossing (see accounts of *S. standfussi*, *S. risii*, etc.), show that the same rule holds good with hybrid *Saturnias*.

(3) Some of the above generalisations, at present resting on only a few instances, can hardly be regarded as more than provisional. It will be interesting to see whether the further results of these experiments, which are being continued by their author, tend to confirm or to modify the conclusions previously reached.

Cross-breeding between local races. Akin to the above experiments are others in which local races of the same species were paired. These gave results in many respects analogous with the foregoing.

1. *Callimorpha dominula* L. ♂ and *do.* var. *persona* Hb. ♀. The crossing of *C. dominula* ♂ with var. *persona* ♀, the latter being a local race found only in Tuscany and Calabria, generally produced issue that, though very variable in the perfect state, bore on the whole a closer resemblance to *C. dominula* than to var. *persona*. In one case, however, a majority of the brood came nearer to the latter, and one individual even went beyond the *persona* type. From 3 to 5 per cent. of the eggs were sterile.

2. *C. dominula* var. *persona* ♂ and *C. dominula* ♀. In this, the reciprocal cross, the resulting perfect insects again varied between the parental types, but, on the whole, came nearer to *C. dominula* than to the variety, though less so than those of the former cross. Of the eggs, from 10 to 15 per cent. were sterile, from which Standfuss concludes that the male of var. *persona* has already diverged from the physiological standard of the species.

The products of each crossing are fertile in both sexes, but Standfuss is unable to say whether they have undergone any diminution in fertility as compared with the parent forms.

3. *Ocnogyna hemigena* Grasl. ♂ and *O. zoraida* Grasl. ♀. These are regarded by the author as local races of the same species, the former inhabiting the Pyrenees and the latter the mountains of Andalusia. The cross-product (called by

Staudinger *O. zoragena*) resembles a large *O. hemigena*. According to Kröning the mongrel issue are fertile *inter se*, but quickly degenerate in size and vitality. This of course may be due to other causes than their mixed origin.

4. *Spilosoma mendica* Cl. ♂ and *do.* var. *rustica* Hb. ♀. The larvæ were nearly always formed within the egg; but in some broods not one, and in others only from 8 to 12 per cent. hatched. In one case, however, as many as 93 per cent. of the eggs gave living larvæ. All the broods suffered severely from disease. The perfect insects, which did not show much variation, diverged only slightly in appearance from var. *rustica*. The males were mostly light-coloured, as in that variety, and the darkest of them were far lighter than any male of *S. mendica*.

5. *S. mendica* var. *rustica* ♂ and *S. mendica* ♀. The reciprocal cross, which failed with Standfuss in consequence of an epidemic among the larvæ, was obtained by Caradja. Pupæ sent by him to Standfuss gave thirty-one perfect insects, seventeen being males and fourteen females. These were more variable than the former cross-product, but on the whole inclined towards var. *rustica*.¹

It is observable that in this case of reciprocal crossing, the influence of the females appears to be equal to if not greater than that of the males. This shows that the generalisation as to male prepotency founded on the case of the *Saturnias* is not of universal application. It will be remembered also that the issue of one of the pairings between *C. dominula* ♂ and var. *persona* ♀ showed female prepotency, though the general result in the case of this and the reciprocal cross tended in the opposite direction.²

Standfuss considers that the present experiments with species and their local races favour his view as to the superior influence of the phylogenetically older form.

Aberrations. Some very remarkable facts are recorded

¹ This corresponds, as has been pointed out by Mr. South, with the result obtained by Mr. Adkin, who effected the same cross in 1889. *Vid. Entomologist*, for August, 1897, p. 206; *Proc. Ent. Soc. Lond.*, 1890, p. xl.

² See also p. 199, *infra*, note.

as to the effect of crossing a sport or aberration with its parent form. The result, which is entirely different from that which follows the crossing of distinct species, or even of local races, may be broadly stated as follows: When an aberration is crossed with its parent form the issue is sharply divided, in both sexes, into specimens of the aberration and of the normal form of the species.

Thus in the dark aberration *zatima* Cr. of *Spilosoma lubricipeda* Esp., there are many degrees from the least dark form of the aberration (ab. *intermedia* Bang-Haas) up to the darkest (ab. *deschangei* Depuis); but no transitional forms occur to bridge over the wide gap between *intermedia* and *lubricipeda*, nor can they be produced by crossing these two. "It seems," so Standfuss expresses it, "as if there were antagonistic characters which cannot coexist in the same individual".¹ Instances follow which will be briefly noticed here. For full details, which are of great interest, the reader is referred to the *Handbuch*, pp. 305-321.

1. *Spilosoma lubricipeda* Esp. ♂ and do. var. *zatima* Cr. ♀. These, crossed by Burckhardt in 1889, gave *lubricipeda*, *intermedia* and *zatima* (*intermedia* being, as just stated, merely a less dark form of *zatima*). Two of these *intermedia* were paired, giving again *lubricipeda*, *intermedia* and *zatima*. In this third generation several pairings were effected, as follows: *zatima* ♂ and *lubricipeda* ♀; *lubricipeda* ♂ and *zatima* ♀; *intermedia* ♂ and do. ♀; *intermedia* ♂ and *zatima* ♀. All these gave *lubricipeda*, *intermedia* and *zatima* in varying proportions, except the cross *lubricipeda* ♂ and *zatima* ♀, from which only *zatima* resulted. A pair of *lubricipeda* from this fourth generation gave a brood of 34 *lubricipeda* and 1 extreme *zatima*.

In all these successive broods, carried on into the fourth year from the date of the original pairing, there were no transitional forms between *lubricipeda* and *intermedia*.²

¹ The bearing of this may be noted on the theory of "determinants". See Weismann, *Germ-plasm*, 1893, pp. 293, etc. See also Bateson, *Materials for the Study of Variation*, 1894, esp. Intro., Sect. iii.; and this Review, 1897, pp. 554-569.

² The above case of cross-breeding between colour-varieties of the same race, in which there are no intermediates and the colours of the ancestors

2. *Psilura monacha* L. ♂ and *do. ab. eremita* O. ♀. In 1893 Standfuss raised a brood from a pair of normal *P. monacha* received from Silesia. This brood contained one female specimen of the dark aberration *eremita*, which was paired with a normal *P. monacha* ♂ from Zürich. The issue consisted of 22 (2 ♂ and 20 ♀) typical *monacha*, 23 (18 ♂ and 5 ♀) typical *eremita*, and 6 forms (5 ♂ and 1 ♀) in which the characters of the two are unsymmetrically mixed, not harmoniously blended. There is no apparent tendency to hermaphroditism. This unsymmetrical mixture resembles that occasionally seen in the male of *Ocneria dispar* L., where also, according to Standfuss, the patches of the female pattern that sometimes appear do not betoken organic hermaphroditism.

3. *P. eremita* ♂ and *P. monacha* ♀. This was a natural pairing found by Standfuss in Silesia. The result was entirely different from that of the former cross, inasmuch as the issue contained every kind of transition between the two parent forms, while a few even transcended the male parent in darkness.

Thus the two specimens of so-called *eremita* in this and the former observation, though externally so much alike, possessed entirely different properties in regard to the power of transmission to descendants. Standfuss explains the apparent contradiction thus: The first *eremita* was a true sport or aberration, and in its case the rule held good as usual. The second, also called "*eremita*" (which did not show the *eremita* characters so well as some of its own offspring), was a link in the chain leading by slight variations to a darker and presumably better protected form of *P. monacha*, which, under the influence of natural selection, is gradually developing itself in certain parts of the range of 195 individuals are known for three generations, seems, like that of *Agria tau* and its aberration *lugens* (*infra*, p. 199), to promise suitable data for the verification of Mr. Francis Galton's law of heredity. According to this law the sum of the ancestral contributions is expressed by the series $(0.5) + (0.5)^2 + (0.5)^3 + \dots + (0.5)^n$, each term standing for a generation. See Galton, *Natural Inheritance*, 1889, p. 134, and "Average Contribution of each Several Ancestor to the Total Heritage of the Offspring," *Proc., Roy. Soc.*, 1897, pp. 401-413.

of the species. It took rank therefore not as an aberration, but rather as a member of a local race, and with this its behaviour accorded.¹

4. *Agria tau* L. ♂ and *do. ab. lugens* Stdfs. Like the dark aberration *zatima* of *S. lubricipeda*, the dark *Agria tau* (called by Standfuss *ab. lugens*) exists in several degrees of development, from the “*ab. fere nigra*” of Thierry-Mieg to the “*ab. nigerrima*” of Bang-Haas. All these different stages of darkening have been obtained by Standfuss from pairings between *A. tau* and *ab. lugens*, but no transitional form between the normal *A. tau* and *ab. fere nigra* has ever been so produced.

In 1888 Standfuss crossed males of *ab. lugens*, which had been interbred for two generations, with females of the normal *A. tau*. In 1889 *lugens* derived from these were employed in the following pairings: *lugens* ♂ and *tau* ♀; *tau* ♂ and *lugens* ♀; *lugens* ♂ and *do.* ♀. The specimens of *A. tau* were in each case of different ancestry from the *lugens* stock. In 1890 two separate pairings were procured of *lugens* ♂ and ♀ from the third of the above broods, so that the perfect insects emerging in 1891 had both parents and all grandparents of the *lugens* type. The results of these experiments were curious, and should be studied in the original account (*Handbuch*, p. 312). Here it may be briefly stated that of the 1889 pairings, the first two (*lugens* ♂ and *tau* ♀; *tau* ♂ and *lugens* ♀) produced about 50 per cent. of each form.²

The third (*lugens* ♂ and ♀) gave 36 per cent. of *tau* to

¹ Compare the result of crossing *C. dominula* with the local race *persona*, and see especially Standfuss's figures, *Op. cit.*, Taf. V. Compare also the case of *A. betularia* and its aberration *doubledayaria* (*inf.*, p. 201), which is strikingly analogous with that of *P. monacha*.

² In both of these reciprocal crosses there is a slight preponderance of forms resembling the *male* parent—in the first instance *lugens*, in the second *tau*. This case is fairly analogous with that of the breed of Basset hounds lately investigated by Mr. Francis Galton (“The Average Contribution of each Several Ancestor,” etc., *Proc. Roy. Soc.*, 1897, pp. 401-413), where the reciprocal crosses of two colour-varieties show a male prepotency in the proportion of about six to five (*loc. cit.*, Table I., p. 409). As in the instance of *tau* and *lugens*, there are no intermediate forms between

64 per cent. of *lugens*. In the 1890 broods, all of whose ancestors were *lugens* for two generations, the proportion of *tau* had fallen to a little over 11 per cent. in one and a little under the same figure in the other. In each of the five cases about twice as many females as males were of the *tau* form. Hence, as Standfuss puts it, it is more difficult to transform the female of *Agria tau* than the male.¹

5. *Grammesia trigrammica* Hufn. ♂ and *do.* ab. *bilinea* Hb. ♀. The female *bilinea*, taken by Gross at Garsten in Austria, laid eggs of which the male parent was presumably a normal *G. trigrammica*. Of the sixty-seven perfect insects that resulted, thirty-eight were *trigrammica* and twenty-nine *bilinea*. There were no intermediates.

6. *Angerona prunaria* L. ♂. and *do.* ab. *sordidata* Fuessl. ♀. This cross, procured by Zeller, gave seventeen *prunaria* and fourteen *sordidata*.

7. *A. prunaria* ab. *sordidata* ♂ and *A. prunaria* ♀. This cross, also obtained by Zeller, gave eighty-four of *prunaria* to sixty-eight of *sordidata*, *i.e.*, as in the reciprocal cross, about 55 per cent. of the type and 45 per cent. of the aberration. In neither of these cases were there any intermediates.

8. *A. prunaria* ab. *sordidata* ♂ and ♀. Among a large brood reared from the eggs of a pair of normal *A. prunaria*, there appeared three males and two females of the aberration *sordidata*. From a pair of these Standfuss obtained thirteen *prunaria* (three ♂ and ten ♀) and forty-two *sordidata* (twenty-four ♂ and eighteen ♀). Again there were no transitional forms.

9. *Amphidasis betularia* L. ♂ and *do.* ab. *doubledayaria* Mill. ♀. A female *doubledayaria* found by Steinert near the "tricolour" and "non-tricolour" hounds, though there are different degrees of development of black in the "tricolours" which may be comparable with the range of variation between "*fere nigra*" and "*nigerrima*" in *lugens*. It is noticeable that in the case of *S. lubricipeda* and *do.* ab. *zatima* (*supr.* p. 197), the reciprocal crosses do not afford evidence of male prepotency. The numbers, however, are small.

¹ Compare the result of the cross between *P. monacha* ♂ and ab. *eremita* ♀. Here the typical *monacha* form was retained by twenty females and only two males.

Dresden produced seventy-five *betularia* (thirty ♂ and forty-five ♀) and ninety *doubledayaria* (thirty-four ♂ and fifty-six ♀). The male parent was doubtless an ordinary *betularia*. Two of the examples classed as *betularia* were darker than the normal, but otherwise no transitional forms occurred. Standfuss is of opinion that even these two need not be regarded as owing their darker coloration to the cross, for it is well known that *A. betularia*, like *P. monacha*, is undergoing a gradually increasing melanism, which is probably protective, in many parts of its area of distribution. The extreme aberration *doubledayaria*, which thirty years ago was known only from Great Britain, has now appeared in Westphalia, the Rhine Provinces, Hanover, Gotha, and lastly in Dresden and Silesia. In several of these places it is becoming more and more common, and in at least some of them it is found side by side with the darkening forms of *A. betularia*, which, though of different nature and origin from the sport *doubledayaria*, are no doubt being preserved and brought up to its level (in aspect) under the influence of natural selection.¹

10. *Boarmia repandata* L. ♂ and *do. ab. conversaria* Hb. ♀. A large brood raised from the eggs of a pair of normal *B. repandata* contained three males and one female of the aberration *conversaria*. This female, which was paired with a wild male *B. repandata*, produced twenty-eight *repandata* (of which ten were males and eighteen females) and six *conversaria* (four being males and two females). The majority of the larvæ died during the winter. Here again intermediate forms were entirely absent.²

From the above experiments in the pairing of normal forms with aberrations and local races, performed or recorded by Standfuss, he arrives at the following conclusions:—

¹ Compare the facts with regard to *Papilio Sarpedon* given by Jordan, ("On Mechanical Selection," *Nov. Zool.*, 1896, p. 431) in illustration of the position that "abnormal varieties show distinctly the directions in which a species is able to develop". Compare also the result of an experiment cited by Tutt, "Melanism," 1891, p. 15.

² Compare the result of an observation by Mr. South, who raised hybrids from the same two forms in 1883 (*Proc. Ent. Soc. Lond.*, 1887, p. xlv.).

1. When the normal form of a species (Grundart) is crossed with a gradually formed local race of the same species, the result is a series of intermediate forms.

2. When the normal form is crossed with a sporadic aberration, the result in many cases is that the issue divides itself sharply between the normal form and the sport, intermediate forms being absent.

Hence, according to Standfuss, the process of species-formation must be gradual; for when two distinct species are crossed, the issue does not split up into the two parental forms as in the case when one parent is a suddenly formed aberration. On the contrary, the behaviour of the issue of two distinct species is very similar in kind to that of a species crossed with a local race or variety which is being gradually established by the accumulation of slight changes. It would seem therefore that although an aberration or sport may be perpetuated by inheritance, it can never acquire distinct specific rank. No doubt however it may, if selected, eventually replace the original form of the species.

To the foregoing considerations it may here be added that these sporadic colour-aberrations seem to have many points of resemblance with the colour-varieties in domestic animals, such as the "lemon-and-white" and "tricolour" of the Basset hounds referred to above (p. 199, note), or the well-known tortoiseshell, tabby and black of cats. The fact that these domestic varieties exist side by side in the same race and even in the same litter, and that true intermediates are rare or absent, seems to suggest that they originally appeared as sports, and that their perpetuation has been ensured or favoured by artificial selection, just as, if Standfuss is right, the dark aberrations of *P. monacha* and *A. betularia* are being perpetuated and multiplied by natural selection. Finally, the well-known case of the "otter sheep" (Darwin, *Variation of Animals and Plants under Domestication*, 1868, vol. i., p. 100), where also intermediates are said to be absent, and other instances on record, show that the phenomenon of sharp division between types in the offspring is not confined to crosses between colour-varieties of the same race, but occurs in other kinds of aberration as well.

F. A. DIXEY.

THE NATURAL HISTORY OF IGNEOUS ROCKS: II. THEIR FORMS AND HABITS.

THE diverse forms assumed by bodies of igneous rock are in a general sense familiar to every geologist; but our conceptions of them are to a great extent based upon generalised and conjectural diagrams, and the subject seems to be one worthy of closer inquiry. In the brief review which follows we shall take note more especially of those features which seem to have a bearing on genetic questions, and the various considerations touched on below will thus fall into place in connection with the former article of this series. As we there remarked a close relation between igneous activity and crust-movements, whether of continent-building, plateau-building, or mountain-building type, so we shall have to observe in what follows that the forms and habits of igneous rock-masses are related to the *kind* of crust-movement experienced by the region in which they occur. Other factors are not without important influence on the behaviour of igneous rock-masses, but the connection of the latter with the geological structure of the country is found to be of a very intimate character, extending in many examples to special tectonic details.

While this relation is more evidently verified in the case of intrusive rocks, there are not wanting indications that similar rules govern the phenomena of surface vulcanicity. The association of volcanic foci and of fissure-eruptions with the mountain-type and the plateau-type of structure, respectively, can scarcely be regarded as a fortuitous coincidence. In making a few remarks on volcanic, before passing on to intrusive, rocks we shall, however, draw attention to some other points.

The most obvious factors determining the forms assumed by extruded lavas are the volume of an individual eruption and the degree of fluidity possessed by the material when poured forth; and the latter must depend both on the chemical composition of the lava and on the temperature

at which it is erupted. Experiment shows that viscosity of a molten lava increases with its silica-percentage, especially when the latter exceeds fifty-eight or sixty, and is also increased by an unusual amount of alumina and by even a moderate proportion of potash. Accordingly we find that acid lavas are very decidedly less mobile than basic ones, and so flow less readily and to smaller distances; and further, that certain intermediate lavas, rich in alumina and potash, are remarkably viscous, as is illustrated by the peculiar dome-like forms assumed by some trachytic and phonolitic eruptions. Again, the water which all rock-magmas contain must contribute to reduce their viscosity, though it does not perhaps materially affect the relative viscosity of different types of lavas. In subaërial eruptions much of it doubtless escapes as steam before the lava spreads out as a *coulée*, but in submarine outbursts the necessary retention of the contained water may considerably modify the conditions of flow. It seems probable that, beneath a rapidly consolidated crust, a lava may travel over the sea-floor more readily and to greater distances than it could accomplish on land. In all such questions the extremely low thermal conductivity of rocks, both solid and molten, must be borne in mind.

A temperature considerably above the melting-point is another condition which must promote the fluidity of a lava. In a rock-magma occupying a subterranean reservoir any noteworthy superheating can, as Becker remarks, scarcely be postulated. Unless the magma differs in composition to a remarkable degree from the encasing solid rock, an accession of heat will melt a portion of the latter rather than raise the temperature of the former. This is confirmed by the fact that in almost all lavas crystallisation has begun prior to extrusion. The relief of pressure incident to the extravasation of the magma must theoretically, however, bring about a superheated condition, and direct evidence of this is furnished by the corrosion and resorption of the contained intratelluric crystals. Other things being equal, the degree of superheating will be proportional to the depth through which the molten magma has risen; and

the deeper the source of a lava of given composition, the more fluid will it be when erupted. The same consideration will of course apply to any magma which rises through a considerable thickness of solid rocks, even though it may fail to make its way to the surface.

Information as to the viscosity of modern lavas, derived from observation of their rate of flow, would be of interest. From the record of a basaltic eruption from Kilanea Becker (12) has deduced a viscosity fifty or sixty times as great as that of water. The data available for such calculations seem to be few and wanting in precision, and are confined, of course, to subaërial eruptions. As regards the diminution in viscosity due to relief of pressure, the experiments of Barus go to minimise its importance. He finds that the diminution of pressure must be at least 200 atmospheres (or say 2400 feet of rock) to reduce viscosity as much as one degree rise of temperature would do.

A few words should be said here of the roughly cylindrical mases, sometimes of solid igneous rock, sometimes of fragmental accumulations of material, either volcanic or non-volcanic, which mark the actual vents through which volcanic discharges have reached the surface.¹ Of these so-called *necks* many examples are preserved in the districts of former volcanic activity in this country, and illustrations of numerous cases are given in the two handsome volumes recently brought out by Sir A. Geikie (13). The supposition that the conduits or pipes represented by these bodies have been formed by violent explosive agency is one obviously prompted by the observed facts, but it is of interest to find confirmation of it from a new line of research, as developed in certain memoirs by Daubrée (14). The typical instance considered by him is that of the well-known necks in the diamond-fields of South Africa. Seventeen of these are known situated on a straight line some 125 miles long. Each of these has a generally

¹ In a recent paper Captain A. H. McMahon notes on the northern border of Baluchistan a "huge natural pillar" of trachytic ash having a diameter of 100 yards at the base and rising over 800 feet (*Quart. Journ. Geol. Soc.*, vol. liii., p. 293, 1897).

cylindrical form, the width usually diminishing downward. The bounding walls exhibit a remarkable vertical striation. The strata traversed have experienced no alteration at the contact, except that they are upturned. The contents of the pipe are fragmental materials, mainly an ultrabasic breccia, which is the matrix of the diamonds. The phenomena here summarised have been imitated experimentally by Daubrée by the use of high explosives, and his results are, as he shows, of great geological importance. Their application is not only to the drilling of cylindrical apertures, but also to the comminution of rocks to produce volcanic dust, the apparent plasticity of rock-masses under mechanical forces, the upthrust of plugs of solid rock through vertical perforations, and other geological phenomena. The author named believes that some trachytic domes must have been erupted in a nearly solid—not even pasty—condition. In any case the connection between “necks” of breccia or agglomerate and those consisting of plugs of solid rock is a close one, the latter being doubtless due in many instances to a neck formed of fragmental materials having subsequently been invaded by an uprush of molten magma. The essential feature of a true neck, as contrasted with the plug-like intrusions to be mentioned below, is that it represents what has been an actual channel of communication between the interior and the exterior of the earth’s crust.

Proceeding then to the forms of intrusive rocks, we consider first, and more particularly, those characteristic of regions where the disturbances of the earth’s crust have not been of a kind involving lateral thrust. The intruded sheets, conveniently designated *sills*, are not confined to areas exhibiting this type of structure, but they certainly attain their highest development and show their greatest regularity among flat strata, or more accurately, among strata which have not experienced any marked folding referable to the period of the intrusions or earlier. One obvious reason for this is that folding would tend to seal the original partings of the strata in the limbs of the folds, while concurrent fracture would open an easier channel for the molten magma. The absence or rarity of dykes in many

districts of regular sill-formed intrusions is in accord with this. The sills themselves have presumably been fed by dykes, but these are in the nature of the case rarely exhibited. Again, the sill form seems to be related also to the composition of the magma so injected. It has been remarked by more than one writer that typical sills are usually of basic rocks, and this observation seems to connect itself with the greater fluidity of basic as compared with acid magmas. The thickness of the "cover" may also perhaps influence the form assumed by an igneous magma injected among stratified rocks. Russell (15) gives reasons for believing that sills have in general been formed at no great depth below the surface at the epoch of the intrusion; and this he explains by the consideration that, the less the energy expended in lifting the overlying rocks, the more will be available for effecting lateral expansion. There must of course be a limit to the operation of this rule, since if the pressure above be insufficient to restrain the fluid magma, the latter will break out. What this limit may be, we cannot at present state; but certain observations, to which reference has been made in a former number of this journal,¹ indicate that under submarine conditions it may be very small. Certain basic sills in Cornwall and in California, showing curious curvilinear divisions, have been supposed to represent injections effected among deep-sea sediments very near to the ocean-floor. Geikie (13) has recently pointed out the wide distribution of this "pillow-structure" among certain basaltic and andesitic rocks in Britain, especially in the Ordovician of the Ballantrae district and Forfarshire, and of Tyrone, Lough Mask, and other parts of Ireland. He ascribed it to the lavas having flowed into water, but in some instances (*e.g.*, at Cader Idris) the hypothesis of intrusion seems also to be a tenable one. In most cases associated radiolarian cherts give evidence of thoroughly deep-sea conditions.

These various points come out more clearly when the characteristics of sills are compared with those of laccolites.

¹ SCIENCE PROGRESS, vol. v., p. 480.

The typical *laccolite*, using the term in its original and strict sense, is associated with the plateau, not the mountain, kind of structure, and the most perfect examples of it have been described from the great plateau region to the west of the Rocky Mountains. Its special feature, as contrasted with the other types of intrusion to which the name laccolite has been extended, is that the strata beneath are undisturbed, while those above are elevated into a dome, this elevation being due directly to the intrusion itself. The simple laccolite thus differs from the uniform sill in its more restricted area and greater relative thickness at its centre, the lenticular mass thinning out rapidly in every direction. In other words, the elevation of the overlying strata, instead of being extended and of small vertical amount, is localised and correspondingly accentuated. The facts elicited above lead us to inquire whether this difference is connected with a higher degree of viscosity in the magma and a greater thickness of superincumbent strata. As regards the former condition at least the answer seems to be in the affirmative, the typical laccolites being of acid and intermediate rocks. Those of Colorado, Utah and Arizona, as described (6) by Whitman Cross, have silica-percentages ranging from 56.62 to 73.50, with an average of 63.84: the percentage of potash varies between 1.97 and 3.95, the average being 3.02. Rock-magmas of such composition would be much more viscous than the diabases and basalts which are so familiar in the form of sills. It is true that Gilbert (16) has recorded laccolites at Twin Butte, in the south-eastern part of Colorado, composed of what he characterises as a basic syenite-porphyry, but no account of this rock has yet been published,¹ and the limited exposures make the form of the intrusions a matter of inference rather than demonstration. The author named rejects any generalisation as to the connection of the laccolitic form with the composition of

¹ If this is the rock from "Two Buttes," of which an analysis has since been given (*Bull. No. 148, U.S. Geol. Sur.*, p. 182, 1897), it seems to be allied to tinguaitite. Though poor in silica, it contains 4.08 per cent. of potash and 6.70 of soda, and its magma would doubtless possess a high degree of viscosity.

the rocks, and suggests that basis laccolites may have been overlooked owing to their not giving rise to salient topographical features; but the facts, so far as they are known, seem to point to a more positive conclusion. Indeed Gilbert in his original memoir on the Henry Mountains laid stress on "the coincidence of the laccolitic structure with a certain type of igneous rock," and based on this his theory, which can scarcely be maintained at the present day, that the behaviour of the intrusions depended upon "the relative densities of the intruding lavas and of the invaded strata". As regards the question of load, it seems to be generally agreed that typical laccolites were in all cases formed under a very heavy cover of overlying strata. The stratigraphical evidence pointing to this conclusion is reinforced by the results of Cross's petrographical study of a large suite of specimens. Another point noticed by Gilbert in the Henry Mountains is an apparent relation between laccolites and the nature of the encasing strata: they are always enclosed by soft beds. This is, however, to be considered not so much a condition of the formation of laccolites as a factor in determining their selection of a horizon. It does not apply to the associated sheets and dykes.

Excluding the modifications observable in laccolitic intrusions among strata either folded or in process of folding, there are still occasional departures from the simple form of the regular dome with flat base. One such special development is seen when the intrusive mass has spread, not along one, but along many bedding-planes, the central mass passing laterally into a number of rapidly thinning sheets. Instead of the ideal mushroom form, there results one which is comparable rather with a cedar-tree. This, according to Holmes, is exemplified in the La Plata Mountains of Colorado, and Geikie has drawn a similar section to represent the relations of gabbro masses injected among the bedded basalts of Skye. In the latter case a connection seems to be suggested between the partial reversion to the sill form and the basic nature of the intruded magma, but the relatively acid composition of the La Plata rocks forbids our laying much stress on this point.

To the sill and the laccolite Russell (15) has added another type of intrusion, to which he gives the name *plutonic plug*, constituting the third term of a genetically related series. As the laccolite differs from the sill in the narrower localisation of the intrusion and of the resulting uplift of the overlying strata, so in the plutonic plug this localisation is carried to an extreme degree, and the form of the igneous rock-body becomes cylindrical rather than dome-shaped. The intrusions taken as typical examples occur in Wyoming and South Dakota, where they have given rise to a number of hills, some having very striking outlines. The hill named Mato Tepee is of columnar shape, rising almost vertically to a height of more than 600 feet above the platform at its base, and composed of igneous rock thrust like a plug through the strata of the district. The latter are horizontal and undisturbed, even up to the very base of the tower. Other examples are more or less completely exposed, and in some localities, as at Little Sun Dance Hill, there is only a dome-like elevation of the strata to indicate a plug entirely concealed. Comparison of the different cases makes it clear that the igneous magma never reached the surface, and in other respects the intrusions differ entirely from volcanic "necks". The texture of the igneous rocks gives evidence of consolidation at a considerable depth, and the geological relations of the district warrant the same conclusion. The arching of the strata over the concealed plug of Little Sun Dance has been effected without fracture, which argues a very great superincumbent load, and the absence of dykes throughout the region is significant in the same sense. As regards the petrographical character of the intrusions, there is some obscurity in Russell's account, but the rock of Mato Tepee has recently been examined by Pirsson. It is of phonolitic composition, with 61·08 per cent. of silica and 18·71 of alumina. We may safely infer that the magma was of a highly viscous nature, probably comparable with those which in volcanic outpourings formed the trachytic lava domes of Auvergne and the phonolitic protrusions of that and other districts, to which may be added the curious puy-

like domes rising through and above level flows of leucitophyre and leucitite in the Leucite Hills of Wyoming, recently described by Kemp (17).

Returning to the laccolite, it may be observed that even in the High Plateaux of the West, as the Rocky Mountains are approached, the regular lenticular form of intrusion gives place to what has been termed an *asymmetric laccolite*. This is seen in Mount Marcellina and other examples in the West Elk Mountains of Colorado. The intrusion of Mount Axtell is described as "a sheet greatly thickened at one extremity". In some cases such a form is seen to stand in direct relation with a sharp monoclinal fold, which may pass into a fault in an upward direction. Here it is no longer possible to regard the disturbance of the strata as the direct result of the igneous intrusion, but we may perhaps consider the folding and the intrusion as closely connected results of the same set of forces in the earth's crust. In the Rocky Mountain belt itself, for instance in the Leadville district of Colorado, as described by Bulkley (18), we go a step farther. The Palæozoic strata are here thrown into large folds, in which occur intrusive bodies of rock comparably petrographically with the typical laccolites of the High Plateaux to the west. To the lateral thrust which has affected the strata must be ascribed not only the folding, but also the formation locally of gaping cavities along the bedding-planes, into which the igneous magma has been forced. It is not probable that actual vacant spaces were ever formed, the inflow of the magma being presumably concurrent with the process of folding. If, however, we are to separate these two results of a common cause, we must regard the disturbance of the strata as the occasion of the intrusion, not as its consequence.

From the point of view just indicated we perceive an essential distinction between the original laccolites of Gilbert and the lenticular intrusive bodies in *folded regions* to which the title has been extended by some writers. With reference to the disturbance of the associated strata, the one type is an antecedent, the other a consequent phenomenon. A distinctive name for the latter type is still a desideratum.

The nature of the intrusion itself and its relation to folding were made clear by Lossen (19) in his researches in the Harz district, and have been generally recognised. Examples of lenticular intrusive bodies of diabase following the bedding of the strata and clearly determined by folds have been pointed out by Watts at Corndon and other places in the Shropshire district. In some parts of North Wales the maps of the Geological Survey show very evidently how the diabases have been injected mainly along the crests and troughs of the folds, that is, in the places where the strata would tend to part in consequence of the lateral thrust. Here the individual intrusions attain no great thickness, being rather of the nature of inconstant sheets. Regarding the more massive lenticles as the analogue in a mountain-region of the typical laccolite, these more attenuated forms correspond with those which in a less disturbed region appear as regular sills. Among folded strata, however, the relative thickness and extent of an intruded mass cannot have the significance which has been ascribed to them in a plateau region, being determined by externally impressed forces rather than by the fluidity of the magma or the depth of cover. It should be observed too that the surfaces of weakness in the strata which guide the injected magma are not always those of bedding, but may be determined by faults, overthrust or underthrust faults, or other structural accidents.

The varied bodies of intrusive igneous rock hitherto referred to are of well defined and clearly ascertainable forms, but the same cannot be said of the masses, often of considerable size, known as *bosses* or *stocks* and consisting generally of rocks of plutonic characters. These large intrusive masses are, as a rule, clearly transgressive as regards their exposed upper surfaces, but the form and relations of their deeper parts are necessarily matters beyond direct observation, and very divergent opinions have been held concerning them. Some geologists have considered a boss, say of granite, as exposed at the surface, to be merely the summit of a great mass, pictured as of roughly pyramidal shape, extending downward with increasing horizontal di-

mensions to an indefinite depth. Others regard an ordinary granitic intrusion as differing only in its greater size and less regularity from other intrusive bodies which can be more completely examined, and comparable in its general shape and relations with a sill or laccolite, or in some cases perhaps with a "plug". The question is closely connected with another one, *viz.*, to what extent a rock-magma under plutonic conditions "assimilates" or incorporates by fusion portions of the neighbouring solid rocks. One extreme view is that intrusive rocks in general have replaced rather than displaced the rock-masses which they have invaded. Pushed to its limit, this hypothesis leaves little or no room for any invading magma at all, and therefore implies that the igneous rock has been derived almost wholly from the fusion *in situ* of pre-existing solid rocks. Such a supposition seems to be scarcely tenable in the case of an igneous rock-mass occupying a definite space among surrounding rocks and encircled at most by a limited aureole of metamorphism. It is difficult to believe that a relatively small amount of intruded magma could carry sufficient heat to melt a large bulk of rocks, and equally difficult to account for the narrow localisation of heat if supposed due to other causes. That a molten rock-magma may to some extent and under suitable conditions melt or dissolve its encasing walls and fragments detached from them is well known, and observations which may be referred to on another occasion seem to indicate that in certain cases this action is of considerable importance, but its application is probably very limited. There is no doubt that a channel once formed for an igneous intrusion may be enlarged in this and other ways, the magma penetrating fissures and detaching fragments, which are partly dissolved, partly carried forward or upward. Perhaps something of this kind has occurred, for example, in the Ross of Mull granite as described by Goodchild (20), but such an action cannot be described as an assimilation of the surrounding rocks by the granite magma.

The question may obviously be approached from the chemical side, and here the evidence is decidedly against

“assimilation” as a factor of importance in the making of plutonic or other igneous rocks in general. Any considerable incorporation of quartzites or grits should give rise to an igneous rock of much more acid composition than any with which we are acquainted, and similarly the taking up of calcareous rocks should show itself in an abnormal content of lime. As regards the latter, Brögger (11) takes the instance of a large granite intrusion among the Lower Palæozoic formations of Southern Norway. The strata, which abut on the granite, and are truncated by it, are accurately known; most of them are calcareous, and it is calculated that the mixture obtained by melting them together in the proper proportions would have $24\frac{1}{2}$ per cent. of lime. The granite, however, contains only $\frac{1}{2}$ per cent. This is not too large an amount for an ordinary granite; but even assuming the original magma to have been free from lime, it is seen that it cannot have taken up more than one forty-eighth part of its own mass from rocks like those seen in contact with it. Brögger asks, what then has become of the prolongation of the strata cut off by the granite? Since the portions cut off have not been incorporated in the magma, they must exist beneath or beyond the granite. In other words the, granitic body must have a concealed under surface, and be an irregular sheet or lens. The argument is evidently one not restricted to the particular case discussed.

We shall conclude these notes on the morphological characters of igneous rock-bodies by drawing attention to certain peculiarities well exhibited by the Tertiary dykes and sills in the Western Islands of Scotland (13, 21).

One noticeable feature is the tendency of these igneous intrusions to follow lines already marked out by earlier intrusions of like material, and probably from the same source. In this way arise *multiple* dykes and sills. Many basalt dykes in the volcanic area of Skye, for instance, are not simple bodies corresponding in each case with a single injection of the basic magma, but are double, triple or perhaps tenfold, consisting of several distinct dykes in juxtaposition and representing as many successive injections. Not in-

frequently this can be verified, when the edges of the component dykes are indicated by compact selvages, bands of amygdules, etc. ; and we may suppose that other cases are overlooked in the absence of such decisive evidence. Apparent bifurcation is often due to the separation of two dykes or sills which for some distance have run in contact with one another.

We have assumed the several injections which go to build up a multiple intrusive body to belong to a single rock-type, but mention must also be made of the modifications in form and habit produced by the association of different rock-types in *composite* multiple dykes and sills. Good illustrations are again afforded by the Tertiary rocks of the British province, which include both acid and basic types. In the larger intrusive bodies these are represented by granite, often with a micographic tendency, and gabbro, graduating into ophitic diabase, etc. It is well known that the two rocks occur together in the Carlingford district, in Arran, Mull and Rum, in the central part of Skye, and as far away as St. Kilda, and always in close association. In the dykes and sills, however, the relations of the two corresponding rocks—granophyre and basalt—are sometimes of a more intimate nature. In particular, Sir A. Geikie has drawn attention to a remarkable group of composite sills intruded at various horizons in the Lias of Skye, in which the acid and basic rocks exhibit a curiously symmetrical arrangement. Usually there is a central and thicker sheet of granophyre, having in contact with it above and below two thinner sheets of basalt. These composite triple sills are not due to “differentiation in place,” but the acid magma has found its way between two basalt sheets which previously constituted a “double sill”. More complex cases are also met with.

Triple dykes showing a similar association are not infrequent in Skye, and Geikie noted one consisting of a central dyke of spherulitic granophyre, eight or ten feet wide, flanked by two narrower dykes of basalt. Judd had already remarked similar cases in Arran. One at Cir Mhor, which was specially studied, has a central part of

pitchstone-porphyry and quartz-felsite between two dykes of augite-andesite. In an earlier paper by Judd (1874) we find a note recording a section at Mingary Castle, Ardnarmurchan, where "a mass of quartziferous porphyry has been forced between beds of Lower Lias shale, while a later-formed sheet of basalt has evidently taken advantage of the plane of weakness, constituted by their junction, to force itself between them". The author compares this with the case of Puy Chopine in Auvergne, described by Scrope and others.

At certain localities in Skye composite sills of the sandwich-like type referred to seem to be in connection with composite dykes, which have probably been their feeders. It appears thus that the successive basic and acid intrusions have not only spread along the same bedding-plane, but reached that position by the same channel; while facts might be cited pointing to an interval of no great duration between the two injections. Such considerations lend some support to the idea that the two magmas, prior to intrusion, existed together in a common subterranean reservoir. If this idea be granted, there is only a step from successive intrusions of different magmas to a single intrusion of a heterogeneous magma. We cannot premise with any certainty the behaviour of an intruded magma drawn from different portions of a reservoir already differentiated; but phenomena familiar to all geologists clearly point to the existence of what may be termed *heterogeneous* intrusions. An example involving, not acid and basic rocks, but more or less basic varieties of gabbro, has been studied by Geikie and Teall in the Isle of Skye.

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ALFRED HARKER.

GERMINATION OF SEEDS: II. THE HYDROLYSIS AND REGENERATION OF PROTEINS.

THE chemical processes that obtain during germination have been submitted to extensive examination; this application is certainly well deserved, for there is no physiological study that is more likely to elucidate the immensely complex changes that are inseparably connected with phenomena of vegetable life. In the seed is a large store of plastic material which is entirely consumed during development of embryo into seedling, and seedling into plant, so that exceptional facility is granted to all that would ascertain, the manner in which the plant renders this material available for the synthetic transformations to which it is afterwards subjected, as well as the nature of these changes themselves. Since there is no reason for doubting, that the metabolism of germination is, in the main, chemically similar to that metabolism which occurs in mature plants throughout the whole vegetable kingdom, it is almost unnecessary to indicate the superlative importance that is attached to investigations bearing on this question. If the present knowledge of this subject is still deficient in many points, this is rather due to the very considerable difficulties with which the question is beset, than to poverty of research. That which is known of these processes, is very largely due to the most excellent work that has been produced by E. Schulze, and the school of investigators that has emerged from his laboratory at Zürich.

Before the question of proteïnic transformations is entered upon, it is advisable to discuss the system of proteïnic nomenclature, that seems to be now prevalent in this country. This is both deficient and redundant; for, although the term "protein" is used to connote the whole class of compounds more or less resembling the albumins, it is commonly replaced by the word "proteid," which serves also to designate the group of still more complex

substances, such as hæmoglobin, nucleïn, etc. Furthermore, the term "albuminoid" is indifferently utilised to connote both that group of compounds centred round the albumins, and also that associated with gelatin or keratin. The incongruity of this system is obvious, and any attempt to replace it with one more compatible with modern requirements can only be desirable.

A. Wròblewski¹ has recently published a modification of Drechsel's classification, which is adopted in the following pages. The term proteïn is employed by him in widest connotation; he divides proteïns into three classes—(i.) "Eiweissstoffe," (ii.) "Zusammengesetzte Eiweissstoffe," and (iii.) "Eiweissähnliche Substanzen". Although these divisional names may suffice in Germany, it is impossible to effect a concise translation that is satisfactory. It is hence suggested that "Eiweissstoffe" be rendered as "albuminoids," which plan has at least the virtue of rational application, and "Zusammengesetzte Eiweissstoffe" as "proteïds," in which sense the word was employed by its inventor, F. Hoppe-Seyler. But no expression is then left for "Eiweissähnliche Substanzen"; this class is only a refuse-heap into which compounds are swept that will not fit into the other two, and to find a suitable expression is difficult: it is indeed impossible to find any to which many objections could not be made. But since for practical purposes some collective term is necessary, it is suggested that, since gelatin is a prominent member, the word "gelatoids" be provisionally used; there is at least a precedent in the designation "alkaloid".

Wròblewski excludes protamines from proteïns, whereas A. Kossel includes them, and inserts proteoses and peptones, which Neumeister has eliminated.

The reserve-proteïns of hypnotic² seeds are as a rule chiefly in the form of aleurone-grains. This term is used very widely to signify all proteïnic granules that are neither living nor hypnotic. The grains are either more or less

¹ *Ber. d. deut. ch. Ges.*, xxx. (1897-8), p. 3045.

² For the application of this term, see SCIENCE PROGRESS, N.S., vol. i., No. 5, p. 605.

homogeneous optically, or contain inclusions. The latter are principally proteinic crystals, the crystalloids, or such as are usually spheroidal, the globoids; crystals of calcium oxalate also occur occasionally. The globoids may possibly consist of the compound or mixture described by Winterstein,¹ which is the calcium-magnesium salt of an acid phosphoric ester, that yields inosite on decomposition. The aleurone-grains are embedded in a proteinic matrix; both may suffer dissolution during germination, in which case the matrix is composed of necroplasm,² this being the case in certain endosperms; or only the grains may be thus affected, when the matrix consists of hypnoplasm,³ as is the case in the cotyledons of seeds. Previously to dissolution the substance becomes swollen and hyaline.

Inasmuch as appreciation of the various theories, founded on the occurrence of certain nitrogenous compounds during germination, requires knowledge of the products of artificial proteohydrolysis, it is perhaps advisable to devote a few words to the subject.

The question of artificial proteohydrolysis has been attacked by several observers.⁴ As a result of their investigations, it is known that the products fall principally into three groups—(1) nitrogenous bases, (2) aliphatic amido-acids, and (3) aromatic amido-acids. Ammonia is also formed. Examples of the first group are—lysin (diamido-caproic acid), lysatinin (a compound creatin), arginin (guanido- α -amido-valerianic acid), histidin. Examples of the second group are—aspartic acid (amido-succinic acid), glutaminic acid (α -amido-glutaric acid), α -amido-valerianic acid, and leucine (α -amido-caproic acid). The third group is exemplified by—phenyl- α -amido-propionic acid and tyrosine (para-hydroxyphenyl- α -amido-propionic acid). Several other products have been obtained, but they have at present no demonstrated connection with the subject under dis-

¹ *Ber. d. deut. ch. Ges.*, xxx. (1897), p. 2299.

² See SCIENCE PROGRESS, *loc. cit.*

³ *Ibid.*

⁴ Notably by Hlasiwetz and Habermann, Horbaczewski, Schützenberger, E. Baumann, E. Salkowski, Nencki, E. Drechsel, A. Kossel, E. Schulze, S. G. Hedin, and others.

cussion. Different groups of albuminoids and gelatoids yield different products; thus, gelatin, etc., bases and aliphatic amido-acids; peptones, etc., bases, aliphatic and aromatic amido-acids; and proteïns containing sulphur give rise to the same substances as peptones, but also to sulphur-compounds. The protamines are distinguished through yielding only bases.

Proteïds yield albuminoids and various other substances on hydrolysis; of these, nucleïns yield the purine-bases (hypoxanthin, etc.) and nucleïnic and thyminic acids.

The nucleïns and nucleoproteïds are characteristic constituents of nucleoplasm generally, and protamines of that of spermatozoa, whereas in plants the albuminoids and certain gelatoids are chiefly found in the cytoplasm.

The occurrence of asparagin in germinating seeds and seedlings has been long known. It was first discovered by Vauquelin and Robiquet (1805) in the shoots of *Asparagus officinalis*; in 1858 Th. Hartig ascribed to it a general distribution in plants, as did also Boussingault, which statement has been since amply confirmed.¹ The former also regarded it as the form in which the metastasis of proteïns is effected. In 1872 Pfeffer opposed the view of its general distribution, affirming that it was only produced during germination; he too supposed it to be the metastatic form of reserve-proteïns. This view would seem to obtain general acceptance among phytologists of to-day, although it has long been refuted, through the works of E. Schulze and his collaborators, in the form in which it was then promulgated by Pfeffer.²

Von Gorup-Besanez was the first to show that amido-compounds other than asparagin occur in etiolated seedlings of *Vicia sativa*, and his results have been confirmed and extended, a whole series of nitrogenous substances having been discovered that can be regarded as products of proteohydrolysis during germination. The observations have been chiefly made on etiolated plants, because more

¹ For literature on asparagin previous to 1882, see *Die Pflanzenstoffe*, Husemann & Hilger, Berlin, 1882, p. 264.

² Pfeffer does not now retain his old opinion.

favourable material is thus obtained for quantitative determinations, inasmuch as darkness has a considerable retarding influence on the metabolism of nitrogenous compounds. Since a description of the methods of analysis adopted in these investigations would be out of place here, the publications concerned with this question are included in the bibliography and will receive no further notice.

In the subjoined table is a list of some of the plants that have been more systematically examined, with the nitrogenous compounds resulting from proteohydrolysis during germination that have been isolated and identified, as well as with a notice of the etiolated or chlorophyllous condition of some of the seedlings. It has been found that the effect of insolation is to materially alter the quantity in which these bodies are found in etiolated seedlings ; this alteration is in cases so extreme as to effect a qualitative difference :—

PLANT.	PRODUCTS OF PROTEOHYDROLYSIS.	
	ETIOLATED.	CHLOROPHYLLOUS.
<i>Picea excelsa.</i>	Arginin, Asparagin, Glutamin.	Arginin, Glutamin.
<i>Pinus sylvestris.</i>	Ditto.	
<i>Abies pectinata.</i>	Arginin.	
<i>Vicia sativa.</i>	Asparagin, Glutamin, Phenylalanin, Butalanin, Leucine.	Asparagin, Leucine.
<i>Lupinus luteus.</i>	Asparagin, Phenylalanin, Butalanin, Arginin, Leucine.	Leucine, Arginin.
<i>L. alba.</i>	Asparagin, Phenylalanin.	Leucine.
<i>L. angustifolius.</i>	Leucine, Butalanin.	
<i>Soja hispida.</i>	Asparagin.	
<i>Cucurbita Pepo.</i>	Glutamin, Asparagin, Tyrosine, Leucine, Arginin.	
<i>Ricinus communis.</i>	Glutamin, Arginin.	

For the sake of convenience, amido-valerianic acid and phenyl-amido-propionic acid are designated by their shorter names, butalanin and phenylalanin, in this table.

The occurrence of these compounds is certain, all having been isolated and identified, both qualitatively and quan-

titatively, according to strictest chemical canons. But in addition to these there are others, the occurrence of which is very probable, but which are not found in sufficient amount to enable their identity to be irrefragably established. Thus, tyrosine is most probably present in *Vicia sativa* and *Lupinus luteus* (etiolated); amido-valerianic acid in *Lupinus alba* (etiolated and chlorophyllous) and *Soja hispida* (etiolated); phenyl-amido-propionic acid in *Lupinus alba* (chlorophyllous), *Soja hispida* and *Lupinus angustifolius* (etiolated); also leucine and arginin (or similar base) in etiolated *Soja hispida*.

The quantitative relations existing between these compounds will be discussed below, but it may be advisable to mention that glutamin replaces asparagin in certain plants,¹ whereas asparagin replaces glutamin in others,² also that in three of these glutamin and asparagin occur together.³ These plants contain also in small amount some of the amido-acids, tyrosine, phenyl-amido-propionic acid, amido-valerianic acid and leucine.

That these compounds result from chemolysis of the reserve-proteins is certain. For, it has been quantitatively established that their formation is correlated with loss of proteins, and that the N-content of amido-compounds and proteins are at any given period complementary. Further, it has been satisfactorily shown that, although in some cases very small amounts of amido-compounds, nitrogenous alkaloids, and other nitrogenous bodies of unknown nature, occur in those seeds from which seedlings have been reared, the amount of these is wholly insufficient to yield even a small fraction of the substances in question. Again, the preparation of extracts and isolation of the compounds has been so undertaken, that their origin could not be accounted

¹ *Ricinus communis*. *Sinapis alba*. *Brassica Napus*, var. *annua*. *Lepidium sativum*. *Raphanus sativa*, var. *radicola*. *Camelina sativa*. *Spergula arvensis*. *Spinacia glabra*.

² *Triticum vulgare*. *Zea Mays*. *Lolium pratense*. *Arrhenaterum elatius*. *Phleum pratense*. *Papaver somniferum*. *Tropaeolum majus*. *Pinus sylvestris*. *Picea excelsa*. *Helianthus annuus*. *Cucurbita Pepo*.

³ *Helianthus annuus*. *Cucurbita Pepo*. *Picea excelsa*.

for, either through reactions connected with the process, or through putrefaction. Moreover, since these bodies have been obtained without exception from artificial proteohydrolysis, it is inconceivable that they can have arisen in any other manner than by transformation of reserve-proteins, especially since the seedlings were allowed to derive their nutriment solely from these.

There is one point of difference between the products of proteïnic decomposition during germination and that in vitro, namely, that, whereas aspartic and glutaminic acids are produced in the latter, their amides occur in the former, the acids having been never observed in plants. But the difference is a necessary consequence of the dissimilar conditions and only apparent, for in artificial hydrolysis ammonia is formed, this having arisen from the amides which could not exist in the conditions.

Besides these products two other nitrogenous substances have been found in certain seedlings, namely allantoïn (glyoxalyl diureïde) and guanidin (imido-urea), that result probably from proteïnic decomposition. Allantoïn was found by C. Richardson and C. A. Crampton in germinated seeds of *Triticum*, and by E. Schulze and some of his collaborators in germinated buds. Guanidin occurs in etiolated seedlings of *Vicia sativa* and possibly in chlorophyllous ones. Inasmuch as arginin is guanido- α -amido-valerianic acid, it is possible that the guanidin found in *Vicia sativa* may arise from that part of the proteïnic molecule, that would yield arginin in other circumstances.

The series of nitrogenous compounds just described account more or less for the carbon, hydrogen, nitrogen and oxygen of the proteïnic molecule ; it is true that there almost certainly remain some substances composed of these elements not yet separated, but if these are not known in an isolated condition, the class to which they belong is known. But the sulphur still remains to be accounted for. There can be no doubt but that this is finally found in sulphates, for E. Schulze and his collaborators have shown that sulphates are formed during germination of *Vicia sativa*, *Lupinus luteus*, and *Cucurbita Pepo*, and that there is an increase

on the content of sulphates of the hypnotic seeds. The increase is greater in *Lupinus*, as proteïnic chemolysis is more energetic here, and the reserve-proteïns are richer in sulphur. Since the amount of sulphates formed in *Lupinus* approximated finally to that theoretically obtainable from the proteïns, it was concluded that the latter formed the source; but inasmuch as other organic S-compounds were present in the hypnotic seed, capable of yielding the sulphate formed, *Vicia* and *Cucurbita* were examined, and an increase correlative with proteïnic decomposition established. It was noticeable that in *Lupinus* the increase of sulphates during earlier stages of growth did not correspond with that theoretically possible, approximation occurring later; this indicates formation of antecedent thio-compounds. Tammann has found traces of acid sulphuric esters in seedlings, but not more than in seeds; it would be a matter of very considerable interest to ascertain whether these are formed, or perhaps thio-oxyacids.

Nucleïns and nucleoproteïds have been shown by A. Kossel and others to yield purine-bases on hydrolysis, and they have found that these bases have a wide distribution among animals and plants. The production of these substances from plants was first noticed by Schützenberger, who found them in yeast subjected to putrefaction. Their presence has also been demonstrated in seedlings, but the chemical identity of the separate bases occurring has not been satisfactorily determined in all cases, mention being usually made of purine-bases, either because establishment of their nearer nature has not been attempted, or owing to difficulties connected with this. They have been found in etiolated seedlings of *Lupinus luteus* and *Cucurbita Pepo*, young green plants of grass, of *Trifolium pratense*, of *Avena* and *Vicia sativa*, of etiolated *Soja hispida* and malt. Guanin occurs in *Vicia sativa* and *Cucurbita Pepo*, hypoxanthin in *Lupinus* and probably in *Cucurbita*, xanthin probably in *Lupinus*.

The proof that the purine-bases are in these cases existent as such in the material, having been derived from the proteïds contained in the reserve-organs, and not from

those in the growing plant, is not quite satisfactory. For Kossel has shown that boiling water decomposes nucleïns gradually, so that the bases found may have been formed in the preparation of the extracts. But it has been ascertained that the lecithins of many seeds diminish during germination, whereas cholin and phosphates increase, indicating decomposition of the former. It may be that the phosphorus of the lecithins is derived from nucleïns, and since nucleïns are not rapidly decomposed through boiling water, there is a probability that the proteïds of the reserve-organs are utilised during germination.

Salts of ammonium are only formed in small amount and do not appear to play any part in the hydrolysis and metastasis of proteïns. The same is true of nitrates, which are not normal constituents of germinating seeds.

Although the nitrogenous compounds are subjected to such considerable alteration during germination, it has been proved¹ that the N-content remains constant, so that there can be no loss of nitrogen through excretion or formation of volatile compounds.

It has been assumed once or twice thus far, that the proteïnic chemolysis of germination is hydrolysis. The assumption was, however, but an anticipation, for there can be no doubt that the reserve-proteïns are dissolved through hydrolysis and not oxidation, or any other conceivable process. The proof of this is deducible from the following considerations: The artificial chemolysis of proteïns that gives rise to the various compounds mentioned above is hydrolysis, the reaction being effected either through heating the proteïns with strong hydrochloric acid in the presence of stannous chloride, or through the action of barium-hydroxide at high temperatures in a sealed tube. But the products of the proteïnic chemolysis of germination are without exception also found among those resulting from artificial proteohydrolysis. Although many compounds that are produced in the artificial treatment have not yet been demonstrated in seedlings, this does not prove that

¹ By Boussingault, Sachsse, Leclerc, Behrend and Wilsing, Schulze and others.

they do not occur, for they may be present in such small quantities as to defy isolation, or the plant may consume them as soon as they are formed. Also, some of the artificial compounds may not be primary products, but due to secondary reactions. Moreover, the products that result from the oxidation of proteins *in vitro* are not those found in seedlings, although closely connected in some cases, as might be expected. If, too, the process were one of oxidation, it is difficult to see why the increase of sulphates in the early periods of germination of *Lupinus luteus* should not correspond with the amount theoretically obtainable from the decomposed proteins, this approximation being only acquired in the later stages. These facts are quite explicable, if the process is one of hydrolysis. Further, Pfeffer, has stated, that if the reserve-proteins are completely converted into asparagin, a residue of carbon and hydrogen must remain, since asparagin has a percentage-composition that is richer in nitrogen and poorer in carbon and hydrogen than proteins. Now Palladin and Löw regard the matter in this light, and assume that these residues are oxidised to form carbohydrates, which Palladin regards generally as products of proteinic oxidation. But Prianischnikow, relying on the results obtained by Schulze and himself with *Vicia sativa*, shows that, through subtracting from Lieberkühn's formula for protein, of which Palladin makes use, an approximate computation of the total amount of substance formed from reserve-proteins in this case, there is left a small residue of carbon, that need be only hydrolysed to form carbohydrates. Thus the objection of Löw and of Palladin falls to the ground.

But the view of hydrolysis is still further strengthened through observation of the occurrence of proteases in germinating seeds, which exercise *in vitro* a hydrolytic action qualitatively similar to that of pepsin.

When these considerations are justly weighed, but one conclusion can be made, namely, that of Schulze and Prianischnikow, that the process through which reserve-proteins are converted into amido-compounds is hydrolysis, not oxidation.

The position has now been reached in which it is desirable to discuss the agency through which the proteo-hydrolysis is effected. In 1874 von Gorup-Besanez stated that he had extracted protease from several seeds and kiln-dried malt, capable of converting fibrin into peptone. Shortly after Krauch criticised the methods of von Gorup-Besanez, and instituted experiments which in every case gave negative results. About the same time Schulze claimed to have found peptones in the seedlings of *Cucurbita Pepo* and *Lupinus luteus*, the quantity being very small. The knowledge was, however, at that time insufficient to enable discrimination between proteoses and peptones to be effected; the seedlings may have hence contained either or both. More recently Reynolds Green has attacked the subject, and stated that a protease exists in seedlings of *Lupinus hirsutus* and *Ricinus communis* resembling trypsin in its hydrolytic intensity. The proof adduced in support of this is not, however, quite convincing. The most truly satisfactory work on this subject is that of Neumeister, who has lately criticised his predecessors, and examined a large number of seeds and seedlings. He was unable to discover a protease in any hypnotic seed, but found one in seedlings, although not in all. Wherever protease was found peptones were also demonstrated, but these occurred also in seedlings in which no protease could be detected. This was found to be due to pre-existence of peptone in the seeds of the latter. The amount of protease and peptones demonstrable varied in different species. The protease proved to resemble pepsin in so far as the hydrolysis ceased after peptonisation, and the reaction occurred only in acid solution. But it differed from pepsin in being destroyed by hydrochloric acid, requiring an organic acid for manifestation of its activity. Unlike trypsin it was destroyed by alkalies.

Since no protease resembling trypsin in its action has been demonstrated with certainty in seedlings, there is difficulty in accounting for the origin of the nitrogenous compounds detailed above, which undoubtedly arise at the cost of reserve-proteins. Is it possible that the protease

reacts more energetically in the germinating seed, or must this further hydrolysis be referred to that incubus of biologists—protoplasmic activity? The question is at present open.

It has been seen that von Gorup-Besanez' statement, that the proteïnic chemolysis of germination resembles that *in vitro*, has been justified as far as its qualitative aspect is concerned. But the quantitative proportions in which the products arise during germination differ considerably from those that result from artificial decomposition. A glance at the table inserted above and its context shows that, whereas all the plants have yielded some of the characteristic products of proteohydrolysis, others have not been yet identified, although in many cases very probably present in minute amount. Also the proportion, in which those that have been identified occur, bears no resemblance to that obtaining in artificial hydrolysis. Moreover in each plant there is one predominant substance, arginin, glutamin or asparagin, the others being as a rule in quite subordinate amount. Arginin predominates in the *Coniferæ* examined, and in the footnotes to the context of the table is a list of plants in which asparagin and glutamin preponderate respectively, as also one in which such are noticed as contain both amides.

If von Gorup-Besanez' hypothesis is correct, it becomes essential that the qualitative differences and quantitative disproportions between these compounds in different species be explained. The simplest and most apparent interpretation is, that the amido-bodies arise directly in varying proportions, any one albuminoid yielding products that vary with the conditions. This view was put forward by Pfeffer some years back in a criticism of an earlier form of Schulze's present theory. The latter explained the observed phenomena through supposing that the proteohydrolysis of germination resembles both qualitatively and quantitatively that occurring *in vitro*, the products being utilised for regeneration with unlike celerity, inasmuch as the amido-acids on the whole are more adapted for this process than the amides. But since this explanation was

found in itself insufficient to account for the large accumulation of asparagin in the *Papilionaceæ*, Schulze utilised the generalisation of Borodin, that asparagin is formed constantly in metabolism, suggesting that, together with proteïnic regeneration from the amido-acids, katabolic changes occurred in the growing seedling, the amido-acids thus reformed being again consumed, and the amides neglected, so that these gradually accumulated, which accumulation has been experimentally demonstrated. Schulze criticised Pfeffer's explanation, which had at first also occurred to himself, stating that his own view was grounded on knowledge of the chemical behaviour of proteïns and the view generally accepted at that time, and not yet refuted, that the amido-compounds exist preformed in proteïns. Acceptation of this view necessitates the production of amido-substances, in so far as they are of primary origin, in the same quantitative proportions during germination as is artificial hydrolysis. Schulze also pointed out that, even if the view of the existence of preformed complexes should be given up, yet his theory would not fall since based on actual observations and not on theoretical speculations. Schulze does not deny that plants may be able to decompose proteïns in different manners, but says that this question is irrelevant, the point at issue being, whether during germination the products result in varying amount or not, and especially whether asparagin, and glutamin in *Cucurbita*, etc., can arise in such large amount as to contain a greater part of albuminoid nitrogen than all the other products together. Various reasons are given that militate against such a view, and Schulze has in the course of time materially strengthened these, so that at present the only possible conclusion is, that by far the greater part of the amides results from secondary reactions, although they are due indirectly to proteohydrolysis. He has recently shown that arginin, which is so preponderant in the *Coniferæ*, is a direct product of hydrolysis.

As a result of more extended investigations, Schulze has found it necessary to modify his theory somewhat. At an early stage he stated, that several observations pointed to the

supposition that some of the amido-compounds were converted into amides during germination, and this view has been subsequently confirmed. He holds at present that the amido-acids and bases are further decomposed with formation of nitrogenous residues, which unite with anitrogenous compounds to form the amides *synthetically*. The latter are to be regarded as the material from which the proteins of the growing plant are produced, so that they form transitory nitrogenous reserve-substance in etiolated seedlings. This further chemolysis of the amido-compounds with formation of nitrogenous residues cannot of course be ascribed to enzymolysis; here again the physiologist must rely on the omnipotent properties of protoplasm for "explanation".

Thus asparagin can be, to a certain extent, compared with urea, as Boussingault did. Both are partly produced in proteohydrolysis, urea arising from lysatin,¹ while both are produced in major amount through synthetic processes, asparagin (resp. glutamin) from nitrogenous residues of proteohydrolysis and anitrogenous compounds, urea through union of ammonia and carbon dioxide to form ammonium carbamate, and subsequent decomposition of this.

The question of proteïnic regeneration forms still a subject of animated discussion, and no definite statement can be made as to the process. The majority of investigators, among which Boussingault, Schulze, C. O. Müller and Prianischnikow may be mentioned, deny that proteins can be regenerated from amides in the dark, whereas Kinoshita, Hansteen and Zaleski affirm that regeneration occurs. Pfeffer and Borodin ascribe to light an indirect influence, in so far as formation of carbohydrates is concerned, while Müller asserts that its effect is direct. Also, whereas some observers, as C. O. Müller, affirm that carbohydrates are not concerned in the regeneration of proteins, light being the determining factor, others, as Pfeffer, Borodin, Kinoshita, Hansteen and Zaleski asseverate that carbohydrates are essential to the process. E. Schulze apparently holds

¹ Lysatin is perhaps a mixture of equal molecules of arginin and lysin (Hedin); if this be so, the urea would arise from the arginin.

an intermediate position, attributing to amido-acids and amides respectively different functions in germination. He has shown that during germination the completeness and celerity of proteohydrolysis and disappearance of amido-acids is correlated with the quantity of reserve-carbohydrates, the consumption of amides being, however, dependent upon the influence of light.

Hence, as far as present knowledge extends, it may be regarded as proved, that the theory of v. Gorup-Besanez with regard to proteohydrolysis during germination is true; that amides and amido-acids functionate differently in germination, the former only resulting in small amount from direct hydrolysis, being chiefly formed synthetically from products of the further transformation of the latter and anitrogenous substances derived from the reserve-carbohydrates; that this synthesis of amides occurs in the dark: and finally that the earlier stages of proteohydrolysis, at any rate, are due to enzymic action. It is also most probable that proteins cannot be regenerated from amides in darkness, and still nothing can be said as to the rationale of their regeneration in light.

A careful study of the principal contributions to the literature on this subject exemplifies admirably, that by systematic and continuous application most important generalisation can be effected, and phenomena elucidated that had been previously "explained" through invoking the omnipotent aid of protoplasm. An excerpt from a paper of Schulze's is of interest in this respect: "Das im Vorstehenden Mitgetheilte bitte ich zu betrachten als die Gedanken eines Chemikers, welcher sich bemüht, die chemischen Vorgänge in der Pflanze zu verstehen, ohne dass er ein 'eigenthümliches Verhalten der lebenden Eiweissmoleküle' oder ähnliche Annahmen zu Hülfe zu ziehen braucht."

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SECRETION AND ABSORPTION OF GAS IN THE SWIMMING-BLADDER AND LUNGS.

PART II.—LUNGS.

IN the previous paper, I gave an account of the evidence which proves that free oxygen, and in some cases at least nitrogen, is actively secreted by the epithelium of the swimming-bladder. The present paper will deal with the much more difficult question whether the exchange of gases between the blood and the air present in the alveoli of the lungs is also brought about in whole or part by active secretion or absorption. The fact that morphologically the swimming-bladder is so closely related to the lungs increases, perhaps, the probability in favour of the existence of active secretion or absorption by the lungs. On the other hand, it must be remembered that the stream of oxygen is outwards from the blood in the swimming-bladder, while it is inwards to the blood in the lungs.

Between the air in the alveoli and the blood passing through the capillaries of the lung there is interposed practically nothing but the cells forming the capillary walls and the layer of extremely delicate flattened epithelial cells constituting the lining membrane of the alveoli. As protoplasm is semifluid in consistency, and as gases pass with the utmost readiness through water by diffusion, the most natural supposition from a purely physical standpoint is that the exchange of gases is simply due to diffusion, the epithelium playing merely a passive part in the process. Venous blood, when brought into contact with air outside the body, takes up oxygen and gives off carbonic acid, undergoing what appears to be just the same change as that brought about in blood passing through the lungs. The fact that the change occurs so much more rapidly in the lungs than in blood shaken with air outside the body is accounted for by the enormous surface presented by the alveolar walls, and the minuteness of the subdivision of the blood-stream in the lungs. The diameter of a capillary

may be taken as about .001 mm. One cc. of blood spread out in a layer of equal thickness would cover an area of 10,000 square centimetres, or 11 square feet. Venous blood spread out in such a thin film would almost instantaneously become saturated with air. The change from venous to arterial colour can be seen to occur within a second or two even with far thicker films. It is true that in the case of such films the blood is in immediate contact with the air, while in the lungs there is a thin layer of protoplasm between the blood and the air; but the difference from a purely physical point of view is more apparent than real, since even in the case of the exposed film albuminous liquid surrounds the red corpuscles which take up nearly all the oxygen from the air.

There is thus not the slightest difficulty in imagining a physical mechanism by which the interchange of gases between the blood and the air is effected in the lung. The frequency with which formerly accepted physical theories of other physiological processes have proved on experimental investigation to be insufficient has, however, caused the diffusion theory of respiratory exchange in the lungs to be regarded with considerable suspicion; and I propose to give an outline of the results of the investigations to which these suspicions have given rise.

It is clear that if the diffusion theory be correct, the partial pressure or tension of the oxygen in solution in the blood leaving the capillaries of the lung alveoli cannot be higher than in the alveolar air: also that the tension of carbonic acid cannot be lower in the blood than in the alveolar air. Thus on the diffusion theory in arterial blood the oxygen tension ought always to be equal to or lower than, and the carbonic acid tension equal to or higher than, that of the alveolar air.

The tension of carbonic acid in blood flowing from the vessels was first investigated by Pflüger and three of his pupils¹ (Wolffberg, Strassburg, and Nussbaum), who strongly support the diffusion theory. The apparatus devised by

¹ *Pflüger's Archiv*, vol. iv., p. 465; vol. vi., pp. 23 and 65, and vol. vii., p. 296.

Pflüger for these experiments was his well-known "aerotonometer". This instrument consists essentially of a pair of glass tubes, each of which is closed by a tap above and by mercury below, and is so arranged that a stream of blood led from a vessel of the animal can be allowed to trickle slowly down the internal surface of the glass and escape under the mercury. One tube is filled before the experiment with a gas mixture containing more, and the other tube with a gas mixture containing less, carbonic acid or oxygen than corresponds to the expected tension of carbonic acid or oxygen in the blood. The tubes are kept in a water bath at the body temperature, and the pressure inside them is kept equal to that of the atmosphere. If the experiment is successful, the percentage of carbonic acid or oxygen in the first tube will diminish, and that in the second will increase, so that the percentage found afterwards will be nearly the same in both tubes. Evidently this final percentage expresses the tension of carbonic acid or oxygen in the blood in terms of the atmospheric pressure at the time of the experiment.

By means of the aerotonometer the tension of carbonic acid in the arterial blood of the dog was found by Strassburg to be from 2.1 to 3.8 per cent. (mean 2.8 per cent.) of an atmosphere. Unfortunately there is no means of determining directly the tension of carbonic acid in the alveolar air of an animal breathing normally. It is evident, however, that since the expired air consists of a mixture of alveolar air with the pure air contained in the trachea and bronchi at the end of each inspiration, the alveolar air must contain less oxygen and more carbonic acid than the expired air. If the respirations are shallow the difference between expired and alveolar air will be great, whereas with deep respirations there will be little difference. In man the expired air usually contains about 3.5 per cent. of carbonic acid, and according to the experiments of Loewy¹ the volume of the "dead space" formed by the trachea, etc., is about 140 cc., the corresponding volume of air inspired in

¹ *Pflüger's Archiv*, vol. lviii., p. 416, "Untersuchungen über die Respiration," etc., p. 26.

normal quiet inspiration being about 350 cc. The expired air thus consists of a mixture of about two parts of alveolar air, with one part of pure air; consequently it is necessary to increase by about one half the carbonic tension in expired air to obtain the alveolar carbonic acid tension. The alveolar carbonic acid will therefore be about 5 per cent. of an atmosphere in man. This tension will vary only slightly during the different phases of a respiration, since the volume of pure air introduced into the alveoli at each inspiration is only about a tenth of the total volume of air in the lungs. It is to be regretted that in the experiments from Pflüger's laboratory on the arterial carbonic acid tension in dogs no attempt was made to estimate the carbonic-acid tension of the alveolar air, or even of the expired air, during the experiments. Three analyses are, however, quoted by Wolffberg of the expired air of the dog. These gave a mean of 2·8 per cent. If we increase this value by one half to obtain the alveolar tension, as in the case of man, we get about 4 per cent. This is a good deal above the mean carbonic acid tension (2·8 per cent.) found by Strassburg in the arterial blood, and the latter value is surprisingly low on the diffusion theory. It may have been, however, that the animals on which Strassburg made his experiments, though they were not tracheotomised, were breathing so freely that the carbonic-acid tension in the alveoli was reduced to the values which he found for arterial blood.

Much more satisfactory evidence in favour of the diffusion theory was obtained in another way. The tension of carbonic acid in venous blood from the right side of the heart was measured by the aerotonometer, and found to be on an average 3·81 per cent. In the same animals, which were of course tracheotomised, the air supply to a portion of one lung was entirely cut off, and a sample of the gas withdrawn after a few minutes by means of an instrument known as the lung-catheter. On the diffusion theory the tension of carbonic acid in the gas contained in this blocked-off portion of lung ought evidently to become equal after a short interval of time to the tension of the same gases in

the venous blood. It was found by Wolffberg and Nussbaum that on an average this was the case, although in individual determinations the tension found was sometimes a good deal higher in the lung and sometimes in the blood. The average carbonic-acid tension in the blocked-off lung was 3·84 per cent., as compared with 3·81 per cent. for the venous blood.

These results evidently afford very strong support to the diffusion theory. It may be pointed out, however, that, apart from a probable source of fallacy due to the bronchial circulation, there appears to be clear evidence that the venous blood coming to the lungs was not normal venous blood, but blood in a more or less arterialised state, the difference being due to the disturbance produced by the tracheotomy and the introduction of the lung catheter. In animals which had not been tracheotomised Strassburg found that the carbonic-acid tension in the venous blood varied from about 5 to 6·5 per cent., and was thus always much higher than in the experiments just mentioned. On the hypothesis that the lung epithelium acts like that of other excretory glands, it is reasonable enough to suppose that the excretory activity would not come into play with a low carbonic-acid tension in the venous blood, or even that the epithelium might oppose the tendency of carbonic acid to escape by diffusion. The kidney epithelium, for instance, seems to regulate in the most delicate manner the percentage of water, chlorides, sugar, etc., in the blood. Thus in the case of chlorides the kidney epithelium either actively opposes their natural tendency to escape by diffusion and filtration or actively excretes them, according to their scarcity or excess in the food taken in. The normal high percentage of chlorides in the blood is thus kept very steady. A certain tension of carbonic acid in the tissues of the body may be as requisite to health as a certain tension of chlorides; hence the fact that under some conditions the evidence is distinctly against an active excretion of carbonic acid by the lungs does not exclude the hypothesis that they act towards carbonic acid like true excretory glands.

In the experiments of Pflüger and his pupils very little

attention was paid to the oxygen tension of the blood. The value of 3 to 4 per cent. obtained by Strassburg for the arterial oxygen tension was only minimal. In a later series of experiments Herter¹ obtained a much higher value, 10·4 per cent., but even this was only minimal.

The subject of the cause of gaseous exchange between the blood and the air of the lungs was next taken up by Bohr.² The general principle of his apparatus was the same as that of Pflüger's aerotonometer, but the blood was rendered incoagulable by a previous injection into the circulation of leech extract or "peptone," and could thus be allowed to flow continuously through the vessel containing the gas mixture and back to the animal. The object of this arrangement was to make it possible to obtain complete equilibrium between the tensions of oxygen and carbonic acid in the gas mixture and in the blood. During the experiments the amount of air per inspiration and the average composition of the expired air was determined by means of a special apparatus. The volume of the dead space formed by the contents of the tracheal cannula and trachea itself was also measured, so that it was possible to estimate the composition of the expired air at the bifurcation of the trachea. From his experiments Bohr concluded that the oxygen tension of arterial blood is frequently above, and the carbonic-acid tension frequently below, that of the expired air; and he draws the inference that the lungs are capable of actively secreting oxygen inwards and carbonic acid outwards.

Still more recently the subject was again taken up by Fredericq,³ whose experiments were made on the same general principles, although he did not determine the composition of the expired air. His experiments showed distinctly that the oxygen tension of the blood from an artery varied from about 8 to 14 per cent. of an atmosphere, while the carbonic-acid tension varied from 1·4 to 4 per cent. The oxygen tensions found by Fredericq were thus con-

¹ *Zeitschrift für physiologische Chemie*, vol. iii., p. 98.

² *Skand. Archiv für Physiologie*, vol. ii., p. 236, 1891.

³ *Archives de Biologie*, 1896, p. .

siderably lower than those usually found by Bohr, and were also lower than in expired air. His conclusion is that diffusion explains the exchange of gases between the blood and the air of the lungs, but that in the case of oxygen there is not time for the tensions in the blood and alveolar air to equalise themselves. Thus, to take an example, in experiment 5 a tension of 2.45 per cent. of carbonic acid (maximum value) was found in the arterial blood. Hence the percentage of carbonic acid in the alveolar air would be 2.45 per cent., and the corresponding alveolar-oxygen tension about 18 per cent. The oxygen tension actually found in the blood was only, however, 9.29 per cent. (maximum value). Hence equilibrium had not nearly established itself between the oxygen tensions in the blood and alveolar air. In a further series of experiments,¹ in which the animal breathed nearly pure oxygen, the difference between the tension of oxygen in the arterial blood and that calculated on the diffusion theory for the alveolar air from the carbonic-acid tension was still greater. Thus with 84.6 per. cent. of oxygen in the inspired air an oxygen tension of 56.2 per cent. and a carbonic-acid tension of 5 per cent. were found in the arterial blood. On the diffusion theory supported by Fredericq the oxygen tension of the arterial blood must thus have been 22 per cent. of an atmosphere below that of the alveolar air.

In addition to Fredericq's own experiments a further series from his laboratory were recently published by Weisberger.² The animal breathed into and out of a large bag. The bag and the aerotonometer were filled with the same gas mixture, and at the end the gas in the bag and in the aerotonometer was again analysed. The results were that as regards carbonic acid the gas in the aerotonometer at the end of the experiment in five cases contained from .66 to 2.75 per cent. more carbonic acid than the air of the bag, and in six cases from 3.36 to .18 per cent. less carbonic acid. As regards oxygen in three cases there was from 12.3 to 4.7 per cent. less oxygen in the aerotonometer, and in six

¹ *Arch. de Biologie*, 1896.

² *Ibid.*

cases from 4·35 to ·08 per cent. more oxygen. In no case was there less carbonic acid or more oxygen in the aerotonometer at the end than at the beginning of the experiment. Weisberger considers that the results support the diffusion theory, and explains the higher percentages of oxygen and lower percentages of carbonic acid in the aerotonometer at the end of most of the experiments on the theory that the changes of composition in the air of the aerotonometer occurred so slowly that they lagged behind those of the bag. This explanation is quite a probable one, but it is evident that the results are not such as to add any very distinct support to the diffusion theory.

Fredericq's experiments were much longer than those of Bohr, so as to allow of sufficient time for equilibrium to establish itself between the gas tensions in the blood and the air of the aerotonometer. He calls special attention to the extreme slowness with which equilibrium establishes itself as regards oxygen tension, and criticises Bohr's experiments on the ground that the latter frequently did not allow sufficient time for this to occur. That such was the case in many of the experiments cannot be denied. It is nevertheless impossible to explain away Bohr's conclusions simply on this ground, as in certain experiments the oxygen in the aerotonometer air was found to have *risen* to a higher percentage than in the expired air, or the carbonic acid in the aerotonometer had *fallen* to a lower point than in the expired or tracheal air, or even, in one case, than in the inspired air. The results of the experiments in question may be quoted here.

Exp. No.	PERCENTAGE OF OXYGEN IN				
	Inspired air.	Expired air.	Tracheal air.	Aerotonometer at beginning.	Aerotonometer at end.
I	20·9	19·18	18·11	20·40	20·44
6 (b)	20·9	18·43	13·52	11·64	14·35
10	20·9	19·16	15·87	17·07	17·80
13	19·99	18·78	18·55	19·20	20·67

Exp. No.	PERCENTAGE OF CARBONIC ACID IN				
	Inspired air.	Expired air.	Tracheal air.	Aerotonometer at beginning.	Aerotonometer at end.
6	0	1.68	4.05	6.07	2.25
12	4.85	5.66	5.75	8.50	4.20

These experiments and particularly No. 12, certainly seem to afford most conclusive evidence that diffusion alone does not in all cases explain the interchange of gases in the lungs. On the other hand, it may be objected that the number of results clearly inconsistent with the diffusion theory is still too small to exclude all possibility of mistakes.

It is evident from the irregularity of the results obtained that the aerotonometer method, even in the improved form given to it by Bohr and Fredericq, presents many serious difficulties in practice, one of the chief of which is due to the fact that the animal is not under normal conditions during the experiment. Apart from these difficulties there are certain sources of fallacy which may or may not be serious, but which have not hitherto been excluded in connection with the method itself. In the first place it is by no means certain that the whole of the blood passing through the lungs is really aerated. Geppert and Zuntz,¹ whose authority on such a point cannot be questioned, bring forward very definite reasons for concluding that, during ordinary respiration at least, a small portion of the blood-stream through the lungs probably escapes aeration. If this be the case the results obtained by the aerotonometer may not be the gas tensions of the aerated blood from the lungs, but of a mixture of this blood with some venous blood. Such an admixture as Geppert and Zuntz believe to occur would not very appreciably raise the carbonic-acid tension of the blood in an artery, but might easily reduce the oxygen tension to half. From experiments by Hűfner with hæmoglobin solutions and fresh blood outside the

¹ *Pflűger's Archiv*, vol. xlii., p. 229, 1888.

body we know that the absence of as little as $\frac{1}{50}$ th of the quantity of oxygen present in blood saturated with air would reduce the oxygen tension in the blood from 20·9 per cent. to half this value.

A further probable source of fallacy in the aerotonometer method lies in the fact that as proved by Pflüger a very appreciable amount of oxidation occurs spontaneously in arterial blood within an exceedingly short interval of time after it has left the lungs. The rapid darkening of the blood in consequence of this change can easily be seen in arterial blood collected over mercury, and has also been observed by Pflüger to occur in the blood within the crural artery if the latter be excluded for a few seconds. The occurrence of oxidation, to even a fraction of the extent observed by Pflüger, in the blood on its way from the lung to the aerotonometer would cause most serious fallacy.

The subject of the causes of interchange of oxygen between the blood and alveolar air was again taken up by Dr. Lorrain Smith and myself about two years ago.¹ The method we employed is a new one, and depends on the well-known fact that hæmoglobin combines with either oxygen or carbonic oxide, the carbonic-oxide compound being, however, much the more stable one. When hæmoglobin solution or undiluted blood is brought into contact with a gas mixture containing both oxygen and carbonic oxide, the hæmoglobin combines partly with the oxygen and partly with the carbonic oxide, the final result being dependent, according to a definite and sharply defined law, on the relative tensions of the two gases. Thus if the gas mixture contain 20·9 per cent. of oxygen and ·070 per cent. of carbonic oxide, the hæmoglobin shares itself equally between the two gases. The partition is the same whether, within certain limits, the atmospheric pressure or the temperature is raised or lowered, or whether by the addition or subtraction of nitrogen the percentages of oxygen and carbonic oxide are increased or diminished in corresponding

¹ *Journal of Physiology*, vol. xviii., pp. 201 and 430; vol. xx., p. 497; vol. xxii., pp. 231, 307.

proportions. An increase or diminution of the tension of one of the two gases, that of the other remaining the same, increases or diminishes in exact proportion the relative share of the hæmoglobin combining with the former gas. Thus provided we know the final state of combination of the hæmoglobin, and the percentage or tension of one of the two gases (oxygen and carbonic oxide), in a gas mixture we can infer the percentage or partial pressure of the other. The striking difference of tint between oxyhæmoglobin and carbonic-oxide hæmoglobin in dilute solution renders it easy to estimate colorimetrically the relative proportions of the two compounds present in the hæmoglobin of even a single drop of blood.

It is clear that on the diffusion theory when an animal breathes, say, $\cdot 07$ per cent. of carbonic oxide in air the latter gas must continue to be absorbed by the hæmoglobin of the blood until an equilibrium establishes itself, due to the fact that the partial pressure of oxygen reached by the blood passing through the lungs is sufficient to prevent further absorption. This point will depend, firstly, on the percentage of carbonic oxide in the alveolar air, which, after absorption has ceased, will of course be the same as that of the inspired air, and, secondly, on the oxygen tension reached by the blood leaving the alveolar capillaries. If, for instance, the oxygen tension of the blood in the lungs just reaches that of the alveolar air the proportion of carbonic oxide finally absorbed by the blood will correspond, in the case of man, to what would be absorbed on shaking the blood to saturation with an atmosphere containing about 14 per cent. of oxygen and $\cdot 07$ per cent. of carbonic oxide. With such an atmosphere the hæmoglobin becomes just 60 per cent. saturated with carbonic oxide. On the diffusion theory of respiratory exchange, therefore, the blood within the body ought finally to become at least 60 per cent. saturated. We have thus a means of testing the diffusion theory by a method which is free from the probable sources of fallacy connected with the aerotonometer method. Moreover, as the absorption of a moderate proportion of carbonic oxide by the blood causes

no noticeable physiological disturbance, the animal experimented on is practically under normal conditions.

Our first experiments were made on men ; and we obtained the very remarkable result that with about $\cdot 07$ per cent. of carbonic oxide in the air breathed, the final percentage saturation of the hæmoglobin with carbonic oxide, instead of being at least 60 per cent., was only about 33 per cent. The data of a single experiment may be given :—

Exp. No. 5. Percentage of CO = $\cdot 067$ throughout.

After 1 hour 15 minutes saturation of hæmoglobin = 26·6 per cent.

„	1	„	45	„	„	„	29·5	„
„	2	„	11	„	„	„	30·5	„
„	2	„	30	„	„	„	30·5	„
„	3	„	10	„	„	„	30·5	„

Were it the case that carbonic oxide is oxidised to any appreciable extent within the body these results might still be explained consistently with the diffusion theory ; for the low proportion of carbonic oxide present in the blood might be due to disappearance of carbonic oxide within the body. There is, however, evidence of the clearest kind against the occurrence of such oxidation.

A further possible hypothesis is that blood corpuscles do not behave towards oxygen and carbonic oxide in the same way inside and outside of the body. There are, however, no grounds for this supposition, and against it is the fact that the corpuscles of defibrinated blood when re-injected into the circulation appear to perform just the same functions as ordinary blood corpuscles.

It is thus very improbable that any other interpretation of our experiments is correct than that some other factor besides diffusion is concerned in the gaseous interchange between the alveolar air and the blood. Evidently, however, the results can be explained on the supposition either that the oxygen tension of the blood is raised in the lungs to beyond that of the alveolar air, or that in consequence of an active resistance offered by the epithelium the carbonic oxide tension in the blood can never rise so high as in the alveolar air. The latter supposition is very unlikely for the

following reason. We know that gases such as nitrogen or hydrogen, which are indifferent in their behaviour towards ordinary protoplasm, diffuse freely through the lung epithelium, either inwards or outwards. Carbonic oxide seems also to behave towards protoplasm as an indifferent gas. It is poisonous to vertebrate animals only because it combines with their hæmoglobin and thus cuts off the oxygen supply to their tissues; but insects, which are not dependent on hæmoglobin, live perfectly well in an atmosphere in which carbonic oxide is substituted for nitrogen; and even in the case of mammals carbonic oxide is no longer poisonous when the animal is placed in compressed oxygen, so that it obtains in simple solution in the blood sufficient oxygen to render it independent of the oxygen-carrying power of hæmoglobin.

A further reason for believing that the lung epithelium presents no obstacle to the passage inward of carbonic oxide is afforded by the fact that when air containing a small percentage of carbonic oxide is breathed nearly the whole of the gas which reaches the alveoli is absorbed, until the point is almost reached where absorption rapidly ceases in consequence of the state of equilibrium being attained.

As there can thus be hardly any doubt that carbonic oxide diffuses freely through the lung epithelium, we are driven to the conclusion that the oxygen tension is raised in the lungs to beyond that of the alveolar air. To explain the results of the above-mentioned experiments on men we must assume that the oxygen tension rose to an average of 38·5 per cent. of an atmosphere, which is about three times the alveolar oxygen tension.

A further series of experiments made on a number of different mammals and birds have fully borne out the conclusions we arrived at in the case of man. Although in different classes of animals the oxygen tensions found have differed considerably, yet in all cases they have considerably exceeded the oxygen tensions of alveolar air. The following table shows the average results for normal animals:—

Man	-	-	-	-	-	38.5 per cent.
Mice	-	-	-	-	-	23.2 „
Dogs	-	-	-	-	-	21.0 „
Rabbits	-	-	-	-	-	27.6 „
Small birds	-	-	-	-	-	44.6 „

We further found that by increasing or diminishing the oxygen percentage of the air a proportional increase or diminution was caused in the oxygen tension of the blood, but that when a condition was produced in which the animal began to show symptoms of suffering from want of oxygen the proportional difference between the oxygen tensions in the blood and in the alveolar air increased very markedly, indicating apparently that want of oxygen stimulates the epithelium to increased absorptive activity.

On the theory that the lung epithelium plays an active part in the absorption of oxygen it would be expected that if by any means the epithelium could be thrown out of action the laws of diffusion would then come into play alone, and the oxygen tension of the blood leaving the lungs would then fall to that of the alveolar air, or lower. During the course of our experiments we found that a fall of body temperature (which greatly diminishes the activity of all the tissues) causes the oxygen tension in the arterial blood to fall to about alveolar oxygen tension. Lorrain Smith has since found that a similar effect follows a rise of body temperature, in spite of the increased ventilation of the lungs; also that during the very acute infective process brought about in mice by inoculation with a culture of *bacillus pyocyaneus* a similar fall occurs. He has also succeeded in producing the same fall in arterial oxygen tension by exposing animals for a short time to oxygen at high pressure and afterwards replacing them in air. Paul Bert has shown that oxygen at high pressure is destructive to both animal and vegetable life. Hence the fact discovered by Lorrain Smith that it has a local effect on the lungs is in no way surprising. If the exposure is sufficiently long a condition of acute pneumonia is brought about, which causes the death of the animal. A short exposure is, however, sufficient to produce a condition in which the arterial oxygen tension

falls to about that of the alveolar air. The following table gives the average arterial oxygen tensions found under the conditions referred to.

Nature of abnormal condition.	Animal.	Arterial oxygen tension found.	Normal arterial oxygen tension.
Fall of body temperature .	Mice.	15·1	23·2
Rise of body temperature .	Mice.	18·2	„
Infection with bacillus pyocyaneus }	Mice.	15·2	„
Previous exposure to oxygen }	Larks.	14·2	37
at high pressure . . . }	Mice.	15·3	23·2

When instead of ordinary air a mixture rich in oxygen was breathed by mice which had previously been exposed to high-pressure oxygen, it was found that the arterial oxygen was always distinctly below that of the air breathed, and not much more than half as high as in normal animals breathing the same atmosphere.

The results of these experiments appear to exclude the possible view that for some reason or other the carbonic-oxide method gives values which are always too high in the living body. They also afford very strong support to the conclusion that the normal lung epithelium plays an active part in the taking up of oxygen. It seems clear that abnormal conditions, whether acting only locally on the lungs, or on the tissues of the body generally, very readily interfere with the normal activity of the epithelium, just as is known to occur in the case of other secretory glands.

It may further be remarked that experiments with carbonic oxide, apart from the evidence which they furnish in favour of active absorption of oxygen, strongly support the view that diffusion alone is capable of bringing about a condition of perfect equilibrium between the gas-tensions in the alveolar air and the blood passing the lungs ; for even the lungs with damaged or paralysed epithelium give an oxygen tension about equal to that of alveolar air. Fredericq's conclusions that the oxygen tension of the blood leaving the lungs is considerably lower than that of alveolar air would present a good deal of difficulty even on the diffusion

theory ; for if there is not time for gaseous equilibrium to establish itself between the blood and the air during rest, it is difficult to see how the blood could be completely aerated during muscular work, when the amount of oxygen absorbed by the lung in a given time is many times greater, although the alveolar oxygen-tension is about the same. Yet we know from the experiments of Geppert and Zuntz that during muscular work the blood drawn from an artery is if anything more completely aerated than during rest.

Against the probability of active absorption by the lung it might be urged that the production of a higher oxygen tension in the blood than in the alveolar air would be useless, since even at the latter oxygen tension the hæmoglobin would, practically speaking, be completely saturated with oxygen. This reasoning depends on the assumption that the only function of the oxygen absorbed by the lungs is to saturate the hæmoglobin of the venous blood. There is now, however, strong evidence to show that the oxygen has other essential functions to perform, and that at any rate the maintenance of a high oxygen tension is essential to health. We found that any great fall in the normal oxygen tension is accompanied by symptoms which cannot be attributed simply to deficient saturation of the hæmoglobin with oxygen. Possibly the high oxygen tension is in some way connected with the very considerable formation of carbonic acid which, according to the recent experiments of Bohr and Henriques, occurs within the lungs.¹

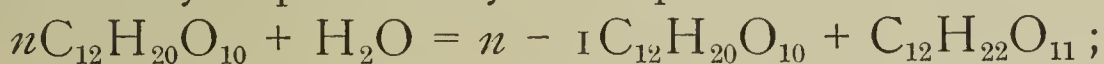
On a review of all the investigations relating to the causes of respiratory exchange between the air in the lungs and the blood it is clear that the balance of evidence is at present strongly in favour of the view that the lung epithelium participates actively in the process. Until, however, the causes of the apparent discrepancies in the results obtained by different observers have been satisfactorily cleared up, we shall do well to regard our conclusions on this, as on so many other questions in physiology, as only provisional.

J. S. HALDANE.

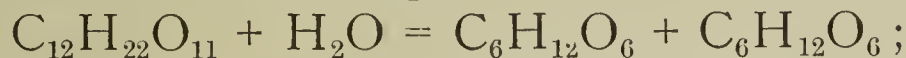
¹ *Archives de Physiologie*, 1897, p. 590.

OXIDASES OR OXIDISING ENZYMES.

THE general course of the action of the enzymes or unorganised ferments is understood to be one of hydrolysis, or decomposition of the bodies attacked by them with the preliminary taking up of water into their molecule. Thus the first action of diastase on starch has been approximately expressed by the equation—



that of invertase on cane sugar



that of emulsin on amygdalin



Though no such equation can be written in the case of the action of pepsin on albumin or other proteids there is reason to think this also is one of hydrolysis. The most notable exception so far known is that of myrosin, which splits up sinigrine into sulphocyanate of allyl, potassic-hydrogen-sulphate and sugar, without the intervention of water.

During the last few years the researches of the French school of physiological chemists have indicated the existence of another group of enzymes, which act by promoting direct oxidation of various substances, chiefly aromatic compounds. As these enzymes appear to be distributed somewhat widely in the vegetable kingdom, the course of ferment action cannot be restricted to the single process of hydrolysis. This need not excite surprise, however, for we have long been familiar with micro-organisms which have an oxidising power. Such are the *Mycoderma aceti*, which forms acetic acid from alcohol, and the organisms in the soil described by Winogradsky and by Warrington, which form nitrous and nitric acids from ammonia.

LACCASE.

Of these *oxidases*, as the enzymes under discussion may be termed, the earliest one recognised was *laccase*, the body

which is concerned in the production of lacquer from the crude sap of the lac tree of South-east Asia.

The existence of this oxidase was first pointed out in 1883 by a Japanese chemist, Yoshida (1), who investigated the latex of that plant and ascertained the nature of the changes occurring in the production of the varnish.

The crude juice is obtained by making incisions into the trunks of several species of *Rhus*, and collecting the viscous matter which exudes. It has the appearance of a nearly white creamy liquid, with a faint odour resembling that of butyric acid. On exposure to air it rapidly changes colour, becoming brown and ultimately black. Spread on a flat surface it dries with a brilliant black lustre. The juice is very difficult to experiment with, as it possesses a very irritating property which affects the skin, causing painful eruptions and sores.

Yoshida states that the juice, known by the name of *urushi*, consists in great part of a peculiar acid, which he has called *urushic acid*, and to which he ascribes the formula $C_{14}H_{18}O_2$. Separated by appropriate methods from the crude latex and dried at $110^{\circ}C$. it forms a dark pasty substance, smelling of the original juice; it is then soluble in benzol, ether, alcohol and carbon-disulphide, but insoluble in water, and has a specific gravity of $\cdot9851$ at $23^{\circ}C$. When exposed to the air it does not dry or show signs of change such as the original latex does.

Besides urushic acid the crude sap contains a certain proportion of gum, and about 3.8 per cent. of a peculiar nitrogenous body, which coagulates on heating to $63^{\circ}C$. If the latex is treated with excess of alcohol the gum and the nitrogenous constituent are precipitated and can be removed by filtration, and the latter can be separated from the former by the action of cold water, in which it dissolves, while the gum only becomes swollen.

This nitrogenous constituent is the source of the enzyme, and on the addition of a small quantity of it to the urushic acid the latter becomes changed into the varnish. If the solution of this albuminoid matter is heated to $63^{\circ}C$.,

the mixture of it with urushic acid undergoes no change. The nature of this nitrogenous constituent has not been clearly established, but it seems to differ from the ordinary proteids by containing a much smaller proportion of nitrogen. Yoshida's analysis of it gives $C_{63.44}H_{7.41}N_{4.01}O_{22.94}$ Ash 1.2 in 100 parts.

From his experiments Yoshida comes to the conclusion that urushi juice consists essentially of four substances, *viz.*, urushic acid, gum, water and a peculiar enzymic matter. The phenomenon of its drying is due to the oxidation of urushic acid $C_{14}H_{18}O_2$ into oxyurushic acid $C_{14}H_{18}O_3$, which takes place by the aid of the enzyme in the presence of oxygen and moisture.

He supports this conclusion by two series of experiments which may be quoted here.

A small quantity of the original juice was put into a covered beaker, and subjected to the regulated heat of a water-bath, the water lost by evaporation being subsequently restored. The heating was carried to different temperatures and subsequently the heated juice was thinly spread over a glass plate and left to dry in a box, the air in which was kept moist. In each experiment the juice was heated for three and a half to four hours and the drying was allowed to take place at a temperature of $20^{\circ} C$.

The results were as follows :—

Temp. of exposure.	Subsequent time of drying.
$20^{\circ} C$.	2 hours.
$30^{\circ} C$.	4 „
$40^{\circ} C$.	$4\frac{1}{2}$ „
55 to $59^{\circ} C$.	24 „
60 to $63^{\circ} C$.	Did not dry.

In the second series of experiments he found that unless moisture was present the latex did not dry; that in moist air it dried in four hours, in moist oxygen in two hours; in moist hydrogen or nitrogen it took thirty-six hours and in moist CO_2 it was dry only after two days' exposure.

It follows from these experiments that the enzyme

works energetically at a temperature of 20° C., but only when oxygen and moisture are both present; a rise of temperature above 20° C. is slowly prejudicial to it, and at 60 to 63° C. it is destroyed. It may be noted that it is at this temperature that the albuminoid matter coagulates.

Yoshida prepared oxyurushic acid from urushic acid by the action of strong chromic acid. He says that so prepared, it exhibits all the properties of the lacquer varnish.

The name *laccase* was given to the enzyme more than ten years later by Bertrand (2), who made further investigations into the peculiar behaviour of the latex and who has ascertained several additional facts about the enzyme.

In the main he confirms the earlier work of Yoshida as to the constituents of the juice. The body described as urushic acid he prefers to term *laccol*, but he has not examined it minutely on account of its deleterious properties.

He prepared the enzyme by treating the latex with a large excess of alcohol, which precipitated a gummy substance, and he purified the latter by redissolving it after filtration, and again throwing it down by the addition of ten volumes of alcohol. It separated out in white opaque flakes which yielded on hydrolysis a mixture of galactose and arabinose.

The enzyme was extracted from the gum by treatment with cold water.

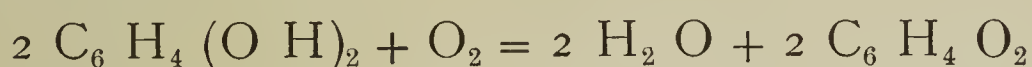
In the natural juice the laccol exists in the form of an emulsion, the latter being probably due to the presence of the gum.

The laccol when separated from the latex by solution in alcohol and kept from the air, remains unchanged. If a little water is added to the solution in alcohol, a white emulsion results, which keeps for a considerable time unaltered; but if for the water a solution of laccase is substituted, the resulting emulsion turns brown at once and rapidly becomes black, especially if air is admitted.

With a boiled solution of laccase no such change can be observed.

So far as Bertrand has investigated the properties of laccol, it appears to be allied to certain polyatomic phenols. On this account he has examined the action of laccase on several of the latter, especially hydroquinone and pyrogallol (3). When the former is submitted to its influence the colour of the solution quickly becomes rose-red, and after a short interval crystalline scales with a green metallic lustre appear, the quantity rapidly increasing. When this operation is carried out in a sealed tube the oxygen present is absorbed almost completely. The liquid gives off a strong characteristic odour, and after the solid matter is separated, quinone can be extracted from it by shaking with ether. The precipitate is quinhydrone.

In the absence of the laccase, or if that employed is previously boiled for a few minutes, the hydroquinone does not absorb oxygen, nor undergo alteration. The hydroquinone is therefore oxidised by the free oxygen under the influence of the laccase.



The colour given to the liquid is due to the formation of the quinone, and some of it, combining with the excess of hydroquinone not oxidised, produces the less soluble crystals of quinhydrone. When pyrogallol is used instead of hydroquinone, similar results are obtained, a precipitate of purpurogalline being thrown down in the form of a powder, which on heating sublimes, forming orange-red needles which are soluble in alcohol and acetic acid.

Laccase attacks many other polyphenols but only those whose phenolic oxhydriles are in the ortho- and para-positions in the benzene ring (4). Those with metaposition are affected only with difficulty. The oxidisability of these bodies by laccase seems to depend on the facility with which they can be transformed into quinones. The monophenols are not oxidised by the enzyme, but it attacks gallic acid and tannin.

Bertrand's observations on the behaviour of laccase at different temperatures do not agree with those of

Yoshida, as he finds it still active after heating it to 70° C. (5).

Bertrand has sought for laccase with some success in other plants and has indicated a rather wide distribution for it (6). In his researches he has employed the guaiacum test and appears to a certain extent to rely upon this method of recognition. This is unfortunate, as most investigators do not find it give entirely satisfactory results. He says that an alcoholic tincture of gum guaiacum turns blue in the presence of air and a little laccase; if much of the latter is present, it turns from blue to green and subsequently to yellow. In most cases however he has confirmed his results by isolating the enzyme and proving its presence by its action. This is really the only satisfactory method of proving its existence. By the two methods conjointly he has found laccase in the roots of the beet, carrot and turnip; in the tubers of the potato and the Jerusalem artichoke; in the tuberous roots of *dahlia*; in certain rhizomes; in the fruits of the apple, pear, quince and chestnut; in the vegetative parts of lucerne, clover, ryeg-rass and asparagus; and in the flowers of *Gardenia*. It may be prepared from these by extraction with water and precipitation of the extract with alcohol. If the tissue is green the extract may be saturated with chloroform and allowed to stand for twenty-four hours, after which the precipitation by alcohol may be carried out.

Rey-Pailharde has found laccase in germinating seeds, especially of plants of the Leguminosæ (7).

The activity of laccase appears to be associated in some way with the presence of manganese. Its ash always contains traces of an oxide of this metal, sometimes as much as 2 per cent. Bertrand (8) states that the activity of a preparation of the enzyme is proportional to its content of manganese.

When prepared from lucerne it is poor in this constituent, and the effect of the addition of a salt of the metal can be easily studied. Bertrand describes a typical experiment on this point. He gathered several kilograms of lucerne at the time of flowering, and bruised them in a mortar, pressing

out the sap, which was then saturated with chloroform and allowed to stand in the dark for twenty-four hours. The juice was next filtered and $2\frac{1}{2}$ volumes of alcohol added to precipitate the laccase. The precipitate was taken up with a little water, the solution filtered and the laccase again thrown down by large excess of alcohol. The final precipitate was collected and dried in vacuo. It contained a mere trace of manganese.

To 50 cc. of a solution of hydroquinone .1 gm. of this precipitate was added, and the whole was agitated for twenty-four hours in contact with air. There was then only a red coloration produced. To a further quantity of 50 cc. of the hydroquinone solution .1 gm. of the precipitated laccase and 1 mgr. of manganese in the form of the sulphate were added together, and in as short a time as two hours crystals of quinhydrone were formed. In the latter case there was an evident oxidation, much more extensive than after twenty-four hours' agitation in the absence of the manganese.

In an experiment so arranged that the absorption of oxygen could be measured, it was found after six hours' agitation with air at 15° C. that with laccase alone .2 cc. oxygen were taken up; with a salt of manganese alone .3 cc. were absorbed, but with both present together 6.3 cc. of oxygen were fixed.

The manganese is thus seen to play a very active part in the ordinary action of the enzyme. No other metal has been found to be capable of replacing it.

Manganese combined with various acid radicals was found in a further series of experiments to have a certain power of causing oxidation of hydroquinone, the protoxide appearing to act as a carrier of the oxygen (9). Comparing the action of these salts of manganese with the conjoint action of manganese and laccase, Bertrand advances the theory that the oxidases can be conceived to be special combinations of manganese with certain proteid bodies containing acid radicals, the latter varying with the particular enzyme. In such combinations the acid radical has just the necessary affinity to keep the metal in solution. The work

of conveying the oxygen would, in Bertrand's opinion, be discharged by the manganese, while the proteid matter would give to the oxidase its other characters, such as are made evident by the action of heat and the various reagents used to identify it.

Whether this hypothesis be accepted or not, it appears from the experiments that laccase at any rate is much assisted in its working by the presence of manganese, if it is not entirely dependent upon its association with that metal in some form.

Besides the plants already mentioned laccase appears to exist in a considerable number of fungi. In these plants the phenomena of oxidation are very prominent, and in consequence of this fact Bourquelot and Bertrand instituted in 1896 an investigation of them, with a view to ascertaining whether laccase or some similar enzyme plays a part in their metabolism (10). As in other cases, at the outset these observers laid considerable stress on the guaiacum reaction, and they found that the liquid that can be expressed from many fungi very rapidly oxidises the tincture with the formation of a blue colour, but that it does not bring about this change if it is first boiled. The reactions of the expressed juice with other bodies than tincture of guaiacum leave no doubt that it contains the same principle as the sap of the lacquer tree. It causes the browning of the laccol prepared from the latex of *Rhus*; it yields crystals of purpurogalline when allowed to act upon pyrogallol, produces quinone and quinhydrone from hydroquinone, and gives a markedly brown colour with gallic acid.

The fungus which yields laccase most readily is *Russula færens* Pers, one of the Basidiomycetes, which is fairly common in woods during the summer. 125 grammes of this fungus, extracted with its own weight of chloroform water yielded 60 cc. of a liquid which was at first pale yellow in colour, but which gradually reddened on exposure to air. When made to act on gallic acid in a closed flask which was constantly shaken, it was found that the oxygen was gradually absorbed, 15 cc. disappearing during the first hour of

action. It gave also the reactions just described with laccol, pyrogallol, etc.

When the extract so prepared was boiled it gradually lost its enzymic powers. Bourquelot and Bertrand say however that it is more resistant to heat than most enzymes, and that to ensure complete destruction the boiling should be maintained for a short time.

When the extract of *Russula* is poured into an excess of alcohol it yields only a small amount of precipitate, but this when separated off gives up the enzyme to cold distilled water. The precipitation of the laccase is not complete however when the extract is treated in this manner.

A very large number of species of fungi have been examined, chiefly belonging to the Basidiomycetes, more than half of which have been found to contain laccase, capable of acting on the aromatic bodies mentioned. Of these the genera *Russula*, *Lactarius*, *Boletus*, and *Psalliota* are the most noteworthy. The Gasteromycetes as a rule contain little if any, and the Ascomycetes and Myxomycetes, so far as they have been examined, are free or nearly free from the enzyme.

Besides working at the effect of laccase on the aromatic bodies as described above, Bourquelot and Bertrand investigated the nature of the changes of colour which supervene when many of the fleshy fungi are cut and the damaged surfaces exposed to the air (11). The tissue of *Boletus* almost instantaneously changes under such conditions, assuming a blue colour, the depth of tint and rapidity of appearance varying somewhat in different species. *Lactarius* becomes violet when wounded, while *Russula* turns first red and finally black.

There have been several theories as to the cause of this change of colour. Schœnbein (12) so long ago as 1856 noticed the phenomenon, and he attributed it to the action of ozone upon a particular chromogen in the fungus, saying that the latter also contains a substance capable of transforming the oxygen of the air into ozone. In 1872 Ludwig made some investigations into the subject, and con-

firmed Schœnbein as to the existence of a special chromogen in the tissue (13).

In the light of the recent work on the oxidases the authors were led to the view that one of the latter probably is concerned in the alteration of the chromogens. According to Schœnbein there is evidently something concerned besides the chromogen, and in his opinion the work effected by the particular constituent in question is the transformation of oxygen into ozone. Whatever it may be it co-operates with the oxygen of the air in causing the oxidation of the chromogen. As this is apparently the part played by laccase in the formation of the lacquer varnish, it seems probable that Schœnbein's hypothetical oxygen transformer is really an oxidising enzyme.

Working on this hypothesis Bourquelot and Bertrand carried out the following experiment. A definite weight of *Boletus cyanescens* Bull. was extracted with boiling alcohol of 95 per cent. concentration, the fungus being cut up as far as possible out of contact with air. The extraction was continued for a quarter of an hour, after which the liquid was cooled and filtered. The alcoholic extract so prepared was faintly yellow in colour, and it contained the substance which normally turns blue on exposure. It retained its colour for a considerable time, even when diluted with water and allowed to stand in contact with air.

To such an extract, diluted with its own volume of water, the investigators added a small quantity of the extract of *Russula* prepared as described above. In half a minute a purple coloration appeared, which passed rapidly into blue. The same effect followed on the addition of a little laccase prepared from the latex of the lacquer tree. If the enzyme was added slowly without agitation the tint was seen to be assumed gradually, the upper layers of the liquid in contact with the air being coloured first and the tint spreading thence throughout the whole.

Hence these investigators infer that the oxidase which can effect these changes is identical with the laccase of *Rhus* and other plants, and that in addition to acting on aromatic bodies, such as hydroquinone and pyrogallol, it

also assists to oxidise the chromogens of certain fungi, especially those which yield a blue or a red colouring matter. The laccase exists in the juice of the fungi side by side with the chromogen, but when the juice is boiled before exposure to the air has taken place the laccase is destroyed, and the chromogen in consequence remains unchanged.

An enzyme similar in many respects to laccase has been described by Piéri and Portier as existing in the gills, palpi and blood of certain molluscs (14).

TYROSINASE.

In other fungi there are different chromogens which do not turn blue on exposure to air, but become red and finally black. Of these, *Russula nigricans* Bull. is perhaps the most conspicuous example. The substance which gives rise to the black colour is almost insoluble in alcohol, but after the fungus has been boiled with this reagent it can be extracted from the residue by subsequent extraction with boiling water. When such an extract is treated with a little fresh cold water extract of the fungus, or a piece of the tissue is added to it, the liquid turns red and after a time black. If the chromogen is extracted from the fungus by boiling water and rapidly pressed, and the exuded liquor filtered and concentrated to a small bulk, it deposits colourless needle-shaped crystals, usually collected together into spheres. They are not soluble in alcohol nor readily in cold water, but they dissolve freely in hot water. They have been identified by Bertrand with *tyrosin*.

Bertrand has observed the same general course of behaviour in the expressed juice of the roots of the beet, the tuberous roots of the dahlia, and the tubers of the potato. In these cases also he has identified tyrosin in the tissues.

The similarity of behaviour to that observed in the cases of *Boletus*, *Lactarius*, etc., points to a similar cause of the change of colour. Laccase, however, has no power to set

up the blackening. Nor will simple oxidising agents bring it about. That it is an oxidation process due to the presence of a special oxidase is asserted by Bertrand, who has named the oxidase in question *tyrosinase* (15).

If to a solution of tyrosin a little of the cold water extract of *Russula nigricans* is added, the mixture becomes at first red, and then later assumes an inky blackness, while finally a black amorphous precipitate settles out. If this is carried out in a glass vessel without agitation, the colour first appears at the surface of the liquid. If it is conducted in a closed vessel from which air is excluded the change of colour does not take place. Nor is the change induced if the extract of the fungus is boiled before being added to the solution of tyrosin. In a closed vessel in the presence of air, the absorption of oxygen can be measured coincidentally with the blackening of the liquid.

Tyrosinase can be extracted not only from *Russula* but from the dahlia and the beet root. It is immaterial which of the three serves as the source of the oxidase, as the effect upon the tyrosin is the same in all cases. *Russula* appears to contain it in greatest quantity. The same mode of extraction can be employed with either material.

In some species of *Russula* the two oxidases so far discussed exist side by side. Bertrand has separated them by the following treatment (5): One and a half kilogramme of freshly gathered *Russula delica* Fries. was reduced to pulp and macerated for half an hour with its own weight of chloroform water at the ordinary temperature. On pressing it, about two litres of a mucilaginous fluid were obtained, to which three litres of 95 per cent. alcohol were added. A precipitate fell which was filtered off. The liquid was concentrated to half a litre by distillation at 50° C. in vacuo, and when so obtained was found capable of acting with considerable energy on pyrogallol and hydroquinone, but to have no effect on tyrosin; it contained therefore only laccase.

The precipitate was washed with 200 cc. of chloroform water and when it was well swollen up, forming a semi-

solution, it was precipitated by addition of 400 cc. of alcohol and pressed dry. It was further purified by a repetition of this treatment. Dried at 35° C. it weighed about 7 gms. This precipitate yielded to cold water after some hours maceration, a principle which oxidised tyrosin rapidly, but had hardly any perceptible action on either hydroquinone or pyrogallol.

Tyrosinase is destroyed at a much lower temperature than laccase, being injured at about 50° C. and more rapidly at higher points; it is possible thus to prepare laccase alone from a mixture of the two, by heating the liquid containing them to 70° C. It then oxidises hydroquinone, but is without action on tyrosin.

Bourquelot (16) has recognised tyrosinase in many genera of fungi, among which may be mentioned *Boletus*, *Russula*, *Lactarius*, *Paxillus*, *Coprinus*, *Psalliota*, *Hebeloma*, *Pholiota*, *Collybia*, *Clitocybe*, *Tricholoma* and *Amanita*; in all these it is associated with laccase, but in the case of *Amanita* the latter enzyme is present only in small quantities.

Besides oxidising tyrosin, Bourquelot (17) has found tyrosinase to act on all the cresols, resorcinol, guaiacol, metatoluidine, xylidine, ortho-, meta-, and para-xyleneol, thymol, carvacrol, and α and β naphthol. He has noted a further peculiarity in its behaviour in that it is effective when dissolved in a mixture of water and either ethyl or methyl alcohol, provided that not more than 50 per cent. of the spirit is present. The alcohols themselves are not affected by it.

ÆNOXYDASE.

Another of these oxidising enzymes has been discovered to play a prominent part in causing a particular disorder in certain wines to which the name "casse" or "cassure" has been applied. According to Bouffard (18) a wine affected in this way loses its characteristic colour, and after three or four hours it contains a red-brown precipitate. If the wine is at rest the decoloration begins at the surface where a thin pellicle of colouring matter forms, and gra-

dually the disturbance spreads to layers deeper and deeper in the liquid, until at last the walls of the vessel are covered by adherent matter, and the liquid is almost decolourised, assuming a moderately characteristic yellow tint. The deposits are formed of the colouring matter of the wine, and are insoluble in solutions of tartaric acid, even if concentrated. The changes are not attended by any evolution of gas. Bouffard says that such wines can be preserved from this disorder by heating them to 60° C. or by the addition of traces of sulphurous acid. The change is not due to bacterial action for it is not hindered by filtration through porcelain, nor by the addition of reagents which are fatal to microbes, such as salicylic acid or bichloride of mercury.

Gouirand (19) has shown that this change is due to some principle which exists in the wine itself. He took some samples of affected wine and after filtering a quantity through porcelain a large addition of alcohol threw down a precipitate of a flocculent character. When this was collected and washed, a small quantity of it added to sterilised sound wines very speedily produced the disorder. This substance was destroyed by heating. In some of Gouirand's experiments he treated samples of sound wines with a small quantity of it, and dividing them into two parts, he heated half to 80° C. In periods varying from twelve to seventy-two hours the disorder was pronounced in the unheated samples, while the controls remained clear and limpid indefinitely. Warming the controls to 60° C. gave variable results; in some it inhibited the action, in others it only slowed its progress. The substance was not affected by heating to 50° C.

When healthy wines were precipitated by alcohol in the same way as the unsound ones, the precipitate had no power of setting up the disorder when added to other samples.

Martinand has ascertained that this substance is present in ripe grapes. An extract of these gives all the reactions of laccase, oxidising hydroquinone, pyrogallol, etc., and loses the power of producing these changes if heated to

100° C. If, however, after cooling, there is added to the extract a little of the precipitate yielded when the juice of fresh grapes is treated with a large excess of alcohol, it regains the power. There is thus present in the grapes themselves, as in the wine prepared from them, a certain amount of this oxidising substance which from its behaviour must be classed with laccase and tyrosinase as an oxidising enzyme.

The name *œnoxydase* has been given to this body. It appears to resemble laccase very closely, but it is not certain that it is identical with it.

Martinand has ascertained it to be present in other fruits than grapes; plums, pears and apples especially may be mentioned. It appears to develop with the ripening of the fruit, unripe grapes containing very little. A good deal seems to be lost in the preliminary process of wine-making, wine itself containing relatively little when compared with the freshly expressed grape juice.

Martinand (21) finds that the *œnoxydase* can be removed from wine by shaking it with ether, which takes from it a body having some of the properties of tannin; this becomes olive-green or yellowish-brown on the addition of ferric chloride, is turned red by alkalies, and gives a white precipitate with albumin but not with gelatin. After the wine has undergone oxidation, most samples do not give up this body to ether, and many others yield only very small quantities of it.

Wine treated with ether in this way, and kept neutral, is not subject to self-oxidation.

The enzyme is possibly associated in the wine, therefore, with this body which is soluble in ether.

Martinand (20) finds that the oxidase is destroyed when its solution is heated to 72° and kept at that temperature for four minutes. Exposure to 55° C. for 1½ hours is also fatal to it. Intermediate temperatures bring about the same destruction after intermediate times of exposure.

Bouffard (22) has observed that the temperature of destruction varies a good deal under different circumstances. He has found that wines beginning to be attacked with

the disorder have been completely preserved by being heated to 60° C., and that warming them only to 55° C. materially helps them to resist it. He has further extracted normal wine by the alcohol method, and side by side with it samples of the same wine after being heated to 60° C. The precipitate in the latter case had no oxidising power, while that in the former was very active. Further investigation showed him that the nature of the medium exercised a great influence on the destruction. When the enzyme was treated in an aqueous solution of neutral reaction it withstood all temperatures below 72.5° C., but when 10 per cent. of alcohol or .5 per cent. of tartaric acid was present destruction was complete at 52.5° C. If double these percentages of alcohol or acid were present, the necessary temperature was reduced 5° C. He agrees with Martinand, however, in saying that it can be destroyed by prolonged heating in neutral media at 60° C.

Dealing with the action of various reagents upon œnoxydase, Bouffard has ascertained that it is destroyed by the action of very dilute sulphurous acid, the necessary amount being .02 gm. per litre of the solution of the enzyme.

Cazeneuve (23) has extracted the enzyme from unsound Beaujolais, and examined many of its properties. He precipitated the wine by excess of strong alcohol, and found the deposit was of a gummy consistency. He took up the gummy precipitate with water, and reprecipitated it with alcohol, collected the deposit rapidly, and dried it in vacuo. He found the precipitate chiefly gum, impregnated with œnoxydase.

In most respects Cazeneuve's results agree with those already quoted, but he finds further that it acts slightly on alcohols and ethers, and on the essences which give wines their peculiar bouquet (24). In its action on the wine he observes that it causes a disengagement of carbonic dioxide, and that after its action there is a diminution of the quantity of alcohol and acid. He attributes the noticeable effects produced to the action of the enzyme on the tannins. As stated above, Martinand has shown that if these are re-

moved by ether the disorder of the wine does not occur. Whether this is due to the removal of the œnoxydase with the tannin, or to the abstraction of the latter only, seems uncertain.

Cazeneuve further establishes a fact which indicates clearly that the disorder is due to the enzyme. He has submitted sound wine to the influence of a current of oxygen for some time, and also to the action of ozone, and he finds that neither process causes "la casse".

The enzyme can be preserved unchanged for some considerable time if dissolved in weak alcohol or in wine which does not contain more than 9 per cent. of spirit. It is, however, rapidly altered by strong alcohol.

It was mentioned above that Martinand had found this oxidase in the juice of apples, pears and plums. Either the same enzyme or a similar one has been described by Lindet as causing oxidation of the tannin in the cider-apple (25). If slices of apple, or a mass of the pulp, or sterilised sponges soaked in the expressed juice are placed under a bell-jar over mercury the material rapidly reddens, and there is a simultaneous absorption of oxygen and an evolution of carbonic dioxide. The phenomenon is the same if the juice in which the sterilised sponges are soaked has been filtered through porcelain, or if antiseptics are added, so that it is evident the changes are not due to the presence of micro-organisms. If boiled juice is used, it remains uncoloured, and there is no exchange of the gases mentioned. The juice may be precipitated by alcohol and the precipitate collected and washed in the usual way, and it is then found to be capable of setting up the changes in boiled juice.

It is of course a common experience that there is a marked difference in the behaviour of the pulp of the apple on exposure to air, according to whether it is raw or cooked. The latter remains uncoloured while the surface of the raw pulp soon turns a reddish-brown, particularly if it is unripe. Lindet holds that the enzyme attaches itself to the tannin, and explains the change of colour seen on wounding the fruit by the suggestion that in the intact pulp the tannin

and the enzyme are situated in different cells, being brought into contact in consequence of the wound. This suggestion seems, however, unnecessary, as the oxidases have been shown to work upon the aromatic bodies they attack only in the presence of oxygen. The access of the latter is only possible when the surface of the pulp is exposed.

ANIMAL OXIDASES.

The oxidases so far described have been discovered chiefly in the vegetable kingdom. It has been mentioned above that recently an enzyme which has similar properties to laccase is stated by Piéri and Portier (14) to exist in the gills, palpi and blood of certain molluscs. They have prepared it by methods similar to those of Bourquelot and Bertrand, and they find it oxidise various aromatic bodies in the presence of oxygen, neither the enzyme nor the gas acting in the absence of the other.

Oxidative processes have long been known to take place in blood when shed and exposed to the air. Claud Bernard first pointed out that under these conditions sugar disappeared, and his results have been confirmed by many subsequent observers. Only within recent years, however, has it been suggested that this disappearance of sugar is due to an enzymic action, but this view is now put forward by several observers.

The action was examined with some completeness by Seegen (26) in 1892, who found that the disappearance of the sugar is not influenced by the presence of chloroform, which prevents the action of living cells and micro-organisms, but does not inhibit the work of enzymes: Seegen found that the exclusion of bacteria by other means does not prevent the glycolysis, and argues in favour of the presence of a sugar-destroying enzyme. The destruction is hindered by a low temperature, occurs very rapidly at 39-40° C., and is inhibited at 54.5° C.

Lepine (27) made some experiments with the pancreas of the dog, which led him to the view that the secretion of a glycolytic enzyme is one of the functions of that

organ. He ground up the pancreas, with aseptic precautions, immediately on removal from the body, and macerated it for two to three hours at 38° C. in water containing .2 per cent. of a mineral acid, and then neutralised the extract with sodic hydrate. To 100 cc. of the liquid resulting, he added half a gramme of glucose and digested it for an hour at 38° C. In a series of such experiments he found that there was a disappearance of sugar ranging from 10 to 50 per cent. A fresh pancreas similarly extracted with water instead of dilute acid, yielded an extract with very little power of causing destruction of sugar. Lepine inferred that from the tissue he used a glycolytic enzyme could be prepared just as similar treatment yields trypsin from the same gland.

He supported his hypothesis by an experiment, in which he compared the glycolytic power of the blood leaving the pancreas during active secretion with that possessed by it when the gland was at rest. He found that during the secretion caused by stimulation of the vagus, blood drawn from the pancreatic vein possesses little glycolytic power, but that the latter becomes considerable in the blood from the same vein during the hours immediately following the cessation of the secretion.

The power of oxidation possessed by blood was examined in 1894 by Abelous and Biarnés, who experimented on its action on salicyl-aldehyde. This body is not oxidised to salicylic acid by the air, nor by distilled water, nor by normal saline solution (a solution of sodium chloride, containing .6 per cent. of the salt). But when defibrinated blood or blood-serum is added to the aldehyde and the mixture kept at a temperature of 37° C. the acid is formed. The oxidation was found to vary in amount with the blood of different animals.

These investigators attribute the action, as did Lepine, to the presence of a specific enzyme, which they say is destroyed by boiling.

They found that besides blood, various tissues of the body possess the same power, notably the testes, the thyroid glands, the liver, kidneys, lungs and spleen. Only when living however can they effect the destruction.

Spitzer (29) also carried out an investigation into the same subject in 1895. He says the glycolytic power is possessed not only by normal blood but that which has been treated with oxalate of potassium, and so rendered uncoagulable. Defibrinated and laky blood also have the same property. Spitzer holds that this glycolysis is not a vital process, but that the blood corpuscles excrete into the serum something which possesses the power of causing it. His work confirms that of Abelous in that he concludes that all living cells possess the property, and that it is one depending on the access of free oxygen.

Spitzer differs from the other workers quoted in not attributing the action to an enzyme, but to the activity of intramolecular oxygen, comparing it with the oxidation produced by hydrogen peroxide and other oxidising agents.

Seegen draws the opposite conclusions, and holds that the enzyme is formed by *post-mortem* changes in the blood. Arthus (30), and Lepine and Barral (31) also have advocated the view that the sugar is destroyed by an enzyme, which they hold to be formed in the white corpuscles. Arthus says that it is destroyed by warming to 55°C., and that it is not present in living blood.

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THE DEVELOPMENT OF BRITISH SCENERY.

"It is a strange thing that the geography of the mother country has never yet been systematically worked out."—J. S. KELTIE, Presidential Address to Section E at the British Association at Toronto, 1897.

IT often happens that the main principles of a subject are first discovered in a region where complications exist which surround the study of that subject with many difficulties, and consideration of the growth of our knowledge of earth-sculpture illustrates this statement. The influence of the running waters of the land, first appreciated by Hutton, as the result of researches in Britain, was very fully proved by a host of our own countrymen, by reference to the facts which they acquired in this island. The complexity of the geological structure of the island prevented the laws of erosion being fully grasped by British geologists, and we owe the first additions to our knowledge of stream-sculpture to the school of American geologists and geographers, working in areas of considerable simplicity, who have supplied us with a very full account of the action of running water; prominent amongst the members of this school stands G. K. Gilbert, whose essay on denudation marks an important step in the study of erosion (1).

The complexity in the distribution of the waterways of a complex geological area may be produced (i.) by the coalescence of a number of drainage systems initiated at different times, or (ii.) by the influence of differential uplifts, and of the characters and distribution of the rocks upon

stream courses which were initiated in a geological sense simultaneously, and it is of importance to determine to which of these causes the complexity of the drainage system of Britain is due. It is generally conceded that the rivers which flow over considerable parts of Eastern England were initiated in Miocene times, or at any rate during the system of earth-movements which culminated in parts of Europe in the Miocene period, consequently, if our whole river drainage was established simultaneously, it must be referred to this date; whereas, if drainage systems were developed in our island at different times some of these must be anterior to Miocene times. It appears to be the opinion of most geologists that the latter supposition is the true one, but it does not seem to be founded upon anything beyond general impressions; a traveller amongst the old rocks of the Scotch Highlands, Cambria or Cumbria, is naturally impressed with the antiquity of the rocks on which he treads, and is unconsciously led to regard the area formed of those rocks as one which has existed as dry land through long geological times. It is desirable, therefore, that we should at the outset consider how far the geological antiquity of portions of our island *as dry land* is proved by the evidence at our disposal.

Every uplift, whether of sea-floor to form land or of pre-existing land to a higher elevation, will tend to influence the drainage and to produce watersheds or divides at first coinciding with the point or line of maximum uplift, though it depends upon the previous inclination of the uplifted rocks whether, after uplift, any definite relationship can be traced between anticlinal axes and watersheds. Now, geologists are fairly agreed as to the periods at which the rocks of the British Isles were affected by important uplifts; and those which require consideration in connection with the subject under discussion occurred at the close of Lower Palæozoic times (mainly in Devonian times), at the close of Upper Palæozoic times (especially during the Permian-Triassic period), in the middle of Cretaceous times, and in Tertiary times (especially the Miocene period) we may speak of them as the Devonian, Permian, Mid-Cretaceous

and Miocene uplifts, without attempting to indicate their maximum periods with greater exactness.

It is obvious that no drainage system can be older than the rocks which cover the area, and in Great Britain there are four large areas and several smaller ones which are unoccupied by Mesozoic rocks; the drainage of these areas may, therefore, have been initiated in Devonian or Permian times. The areas are: Scotland, the Pennine Chain, Wales, and Devon and Cornwall, and it is of interest to consider whether the river systems of these areas are of very great antiquity.

It may be remarked at the outset that if a tract has existed above the sea for long ages and not undergone any further uplift after its emergence from the sea to form high land, the rivers must have reached their base-levels of erosion and produced a plain of subaërial denudation (or *peneplain*, to use Prof. W. M. Davis' term) unless, indeed, the area has existed as a rainless region through these long periods. We cannot, therefore, suppose that the tracts above mentioned have existed as land since Palæozoic times without subsequent uplifts (which would profoundly affect, if they did not completely alter, the drainage-systems) or their present eminences would have been long ago worn away.

In Scotland the main watersheds bear no direct relation to the axes of uplift of the very ancient rocks which occupy so large a portion of its surface and the great thickness of old red sandstone and carboniferous rocks in the depression between the southern uplands and the Highlands indicates the former extension of those rocks far beyond their present limits, whilst evidence of the like extension of still later rocks has been adduced by Prof. Judd, who makes the following most suggestive remark: "In the face of these facts, I believe that it is impossible to avoid the conclusion that the whole of the north and north-western portions of the British archipelago—now sculptured by denudation into a rugged mountain-land—were, like the south and south-eastern parts of the same islands, to a great extent, if not completely, covered by sedimentary deposits,

ranging in age from the Carboniferous to the Cretaceous inclusive ; and that, as a consequence, we must refer the production of the striking and very characteristic features of those Highland districts to the last great epoch of the earth's history—the Tertiary—and very largely, indeed, to the latest portion of that epoch, namely the Pliocene”(2).

Of the three other areas, Wales, which is essentially composed of Lower Palæozoic rocks, possesses a drainage radiating from the Plinlimmon district situated in a synclinal fold of these rocks and accordingly can hardly owe its present drainage directly to the Devonian uplifts, though as Carboniferous rocks nearly surround it on three sides, it might at first sight appear likely that the drainage system was a Permian one ; in Devonshire the drainage is from a watershed coinciding in the main with a synclinal axis produced during the Permian uplift, and this would indicate initiation of the present drainage after Permian times ; the Pennine uplift is also largely Permian, but here the watershed coincides in the main with the dominant anticlinal axis, and we might very well suppose, in the absence of other evidence, that the Pennine rivers were initiated during the Permian period of uplift.

There is, however, other evidence, which suggests a late date for the drainage of the Scotch, Welsh, Pennine and Devon areas alike, and we may briefly glance at this.

Examination of a geological map of Europe will show that the Mesozoic and Tertiary rocks of England occupy the western end of a complex syncline, with an axis directed in a general east and west direction, and the age of the rocks included in this folded system indicates that their uplift took place during the Miocene period of elevation. But the mean trend of the Mesozoic rocks of Britain is nearly north-east and south-west, for the strike approximates to a north and south direction in Lincolnshire and South Yorkshire (the dip being east), whilst the strike is practically east and west in the south of England. This modification in the direction of strike is undoubtedly due to the uplifts of the Mendip and Pennine systems, which have the same general direction as the strike of the newer

rocks in their vicinity, and they acted as "horsts" or barriers of harder rocks which affected the strike of the adjacent deposits. It by no means follows, however, that they were uncovered at the time that the position of the newer rocks was affected by their existence, for we actually find the continuation of the Mendip ridge still buried beneath the newer rocks underneath, and the same is almost certainly the case with the rocks of the Pennine Chain to the south, portions of which now stand out as inliers of older rock through the new Red Sandstone deposits of the central plain of England.

The Mesozoic rocks lying east of the Pennine Chain dip in such a way, that the position of the floor on which they were deposited, would, if the dip were continued westward, lie far above the summit of the Pennine Chain, and yet we find new Red Sandstone running from the west of Cumberland, down west Lancashire to Cheshire, and so into the central plain, and outliers of Lias (Rhætic) near Carlisle and near Wem in Staffordshire, far beneath the level at which they should occur, if the easterly dip were continued farther west. The conclusion seems inevitable that the Pennine Chain was uplifted in Post-Rhætic times, and therefore probably during the Mid-Cretaceous or Miocene periods of uplift, and in favour of the latter period is the existence of Cretaceous rocks in the north of Ireland, and possibly in the Irish Sea.

I have elsewhere argued in favour of the elevation of the Lake district in Miocene times (3), on account of the structure of the district, and suggested its dependence upon the formation of a laccolitic dome.¹

The occurrence of Cretaceous gravels near Buckland Brewer, in the extreme west of Devonshire, suggests that though the Cretaceous rocks, as is well known, thin out and assume shallow water characters when traced towards

¹In the paper alluded to, geological details were largely omitted, but the coincidence of the watershed near Shap with an anticlinal axis should have been mentioned (though well-known to British geologists), as the supposition that the Howgill Fells were uplifted after the initiation of the waterways is largely dependent upon this fact.

Devon from the east, they once covered the district. The deposition of Eocene beds at Bovey Tracey on Pre-Cretaceous rocks however shows that the district was affected by the Mid-Cretaceous uplift (or at any rate by one in Post-Cretaceous and Pre-Eocene times) if the Cretaceous rocks once extended over Devon, but the nature of the Bovey beds indicates the probability of the former extension of similar beds over Devon and Cornwall, and suggests the final great uplift of the Devon-Cornwall mass in Miocene times.

Coming now to the Welsh area, we find Triassic rocks dipping away from it on the north, east and south, and Rhætic rocks on the south, in a manner which suggests the elevation of Wales in Mesozoic or Tertiary times. We have no direct evidence of the extension of Cretaceous rocks over the area, but even if an uplift occurred in Mid-Cretaceous times which prevented the accumulation of the Cretaceous rocks over the area, the Welsh rocks were probably worn down to a peneplain before the great Miocene earth-movements.

Examination of the geology of England in fact indicates that had the Miocene tilt been in an opposite direction, giving the newer strata a westerly dip instead of an easterly one, the Highlands of Britain would be on the east side, the Mesozoic rocks would be denuded there, and the London ridge and similar ridges now buried beneath newer deposits would form a hilly country occupied by the more ancient formations, whilst the west of England and possibly Scotland and Ireland would consist of low ground formed of a peneplain of old rocks, or more probably of Triassic beds and even later deposits, possibly as modern as the Cretaceous and Eocene beds. It must be remembered that even if the west existed as land in Eocene times, the characters of the lignites and basalts of the Western Isles of Scotland and Ireland suggests their former extension as plateaux over much wider areas (4), and they may well have extended over much of the country now occupied by older rocks.

To return for a moment to Wales: The newer rocks which surround the older Palæozoic rocks of the Principality are continued across the Irish Sea and St. George's Channel,

and pass round the older Palæozoic rocks of Wicklow and Wexford, so that, if the sea were dried up, we should probably meet with a dome of old rocks, entirely surrounded by a ring of Carboniferous rocks and in parts bounded by newer formations.

I need hardly say that the proofs of the impress of the present surface features of the whole of Britain during the Miocene uplift can only be obtained as the result of much more work ; I have merely endeavoured to show that Prof. Judd's view must be regarded as, at any rate, quite as probable as the one which supposes that many of our surface features date from very early times.

The movements, whatever their age, produced a general elevation in the west of England as compared with the east, and gave the main English rivers their trend to the east, whilst the subsidiary uplift of the Pennine Chain, and the formation of a syncline to the east of it, determined the Pennines as a subsidiary watershed, lying some distance to the east of the main watershed. The elevation of the lake district dome produced a subsidiary radial drainage in that region, and the Tertiary uplifts in Southern England gave rise to the Wealden drainage and to minor drainage systems situated to the west of it.

We have evidence in the existence of many submerged valleys around our coasts, and also in the interior (the latter filled with drift), that in Pre-Glacial times our land was as a whole situated at a higher level than it is at present, when indeed it formed part of the continent. The drainage to the east of England then flowed into the continuation of the Rhine ; that to the south, to a river situated in the position now occupied by part of the English Channel. Soundings to the west suggest the existence of a deep river valley running from Scotland towards Cornwall, into which the rivers of the west of England, of Wales and of the east of Ireland, flowed as tributaries, and the existence of a deep channel, shallowest about the centre of the uplift of Wales, Wexford and Waterford, points to the existence of this river before the Welsh uplift, and the consequent disturbance of what would otherwise be a radial drainage,

causing the Welsh watershed to lie much east of its proper position.

The drainage established during the uplift (or uplifts) consisted primarily of rivers whose sources lay along the axes of uplift, with their subsequent and obsequent tributaries, such as the Tyne, Tees, Humber and other East Anglian rivers, and the great stream of which the Thames is a beheaded portion (5), partly of antecedent streams which continued to flow for some time across the axes of uplift (as for instance the Scotch-Cornish river on the site of St. George's Channel and the Irish Sea?), and partly of rivers flowing along synclinal depressions, as the lower parts of the Dee, the river on the site of the Severn Estuary and that part of the Thames which lies in the Eocene basin.

It is the task of the English geographer to trace the modifications which have complicated this initial river system, as well as to clear up the story of its initiation; and much work is being done in the direction of elucidating the subsequent complications. I have already referred to the work of Prof. Davis in this connection, and his paper is extremely valuable to the physical geographer, but much similar work has been done in isolated areas, especially by Ramsay, Jukes, Topley, Jukes-Browne, Strahan, Green and others (6); and we learn from Dr. Keltie's recent address to the Geographical Section of the British Association that it is proposed to carry out work of this kind in a systematic manner. He writes: "Taking the sheets of the Ordnance Survey map as a basis, it is proposed that each district should be thoroughly investigated and a complete memoir of moderate dimensions systematically compiled to accompany the sheet, in the same way that each sheet of the Geological Survey map has its printed text. It is a stupendous undertaking, that would involve many years' work, and the results of which when complete would fill many volumes. But it is worth doing; it would furnish the material for an exact and trustworthy account of the geography of Britain on any scale, and would be invaluable to the historian as well as to others dealing with subjects having any relation to the past and present geography of the land. . . . Dr. H. R. Mill

has begun operations on a limited area in Sussex. When he has completed this initial memoir, it will be for the [Geographical] Society to decide whether it can continue the enterprise, or whether it will succeed in persuading the Government to take the matter up." If either of these desirable plans be adopted, I think Dr. Keltie may feel assured that geographers will receive the heartiest support from geologists in carrying on so laudable a work.

Hitherto I have touched only on the trend of the rivers, as instrumental in determining the character of the scenery of inland districts. Many details of scenery also require study. I have already written an article in this Magazine upon Lakes (7); since that appeared the magnificent memoir on the French Lakes from the hands of M. Delabecque has been published (8), and I hope British geographers will not be contented until similar volumes on the Limnology of England have been published.

The scenery of the coast-lines is a subject which also deserves the attention of British geographers, and we may now turn to a consideration of the dominant features of the shores of our island.

The general principles of coast-formation have been stated by Gilbert in his study of the Topographic Features of Lake Shores (9). He observes that "re-entrant angles of the coast are always, and re-entrant curves are usually, places of deposition. . . . Salient angles are usually eroded, and salient curves nearly always. . . some salient angles on the contrary grow by deposition. . . ."

"It thus appears that there is a general tendency to the erosion of salients and the filling of embayments, or to the simplification of coast outlines. This tendency is illustrated not only by the shores of all lakes, but by the coasts of all oceans. In the latter case it is slightly diminished by the action of tides, which occasion currents tending to keep open the mouths of estuaries, but it is, nevertheless, the prevailing tendency."

The outcome of coast erosion and deposition is the production of concave curves usually meeting at a salient angle, and the English coast illustrates the formation of

these on different scales. Of the larger ones, we notice one extending from St. Bees Head to the north-west corner of Anglesea, a second from Anglesea to the south-west of Carnarvonshire, another from that point to Pembrokeshire, south of that a large one passing Lundy Island to the Land's End. Between the Land's End and the north foreland are a number of curves terminated by the following salient points: The Lizard, the Start, Portland, St. Anne's Head, the south of the Isle of Wight, Selsea Bill, Beechy Head, a point north-east of Hastings, Dungeness, and the South Foreland. To the north of the Foreland, we find a curve broken into by the Thames' estuary and extending to the coast of Norfolk, and north of it is a feature unlike any other in England on so large a scale, namely, the great salient curve of Norfolk, which, so far as I am aware, has not been explained. Continuing northward, we again meet with a series of embayments, bounded by salient points, at Spurn Point, Flamborough Head, Whitby and Dunstanborough. It would seem that many of these larger curves are portions of an old coast line, existing during the period of elevation marked by the buried valleys. For instance, Lundy Island, situated on the curve from Pembrokeshire to Land's End, seems to represent a portion of a destroyed coast. Accordingly, we find the curves are not only modified by estuarial expanses, such as Morecambe Bay, and that lying south-east of Carnarvonshire, but a number of fjord-like indentations, indicating depression, run far inland, like the Barmouth estuary, and many of those on the coast of Devon and Cornwall.

An examination of these coast lines in detail will show important connections betwixt the geographical structure and the physical features. Morecambe Bay seems to be determined by the existence of soft new Red Sandstone rocks faulted against the older rocks and the great sweep from Cumberland to Wales is no doubt due to the existence of these rocks.

In later times, the primary curves have been modified by smaller curves. For instance, the great curve from the Start to Portland is modified by the occurrence of minor

loops as Start Bay, Torbay and others to the east, and the same thing is noticed with the other bays.

Enough has been said to show that the topography of coast lines is a fertile subject for research. Two papers have recently been published by Dr. Gulliver (10) and Mr. Vaughan Cornish (11), which bear directly upon the topography of our coasts and further work will no doubt follow.

The shape of England is roughly an isosceles triangle with a base extending from Northumberland to Cornwall and the apex on the coast of Kent. The position of the base is due to the uplift of Palæozoic rocks, to the west and north of England, whilst the position of the two sides is owing to the strike of the Mesozoic and later rocks, with a general northerly trend to the north and a westerly trend to the south, caused by the presence of the Pennine and Devon ridges or horsts. Along the strike of these Mesozoic rocks strike-rivers carved out valleys in the old continental plateau, which after depression were occupied by the North Sea and English Channel, giving England its present outline. As denudation progresses, should no further uplift occur, the Mesozoic rocks of the east and south, and the new Red Sandstone of the Central Plain and the lower parts of the Dee and Mersey basins will be denuded and our island will be broken up into an archipelago of Palæozoic rocks, bearing few or no signs of the possible modern origin of the whole as land.

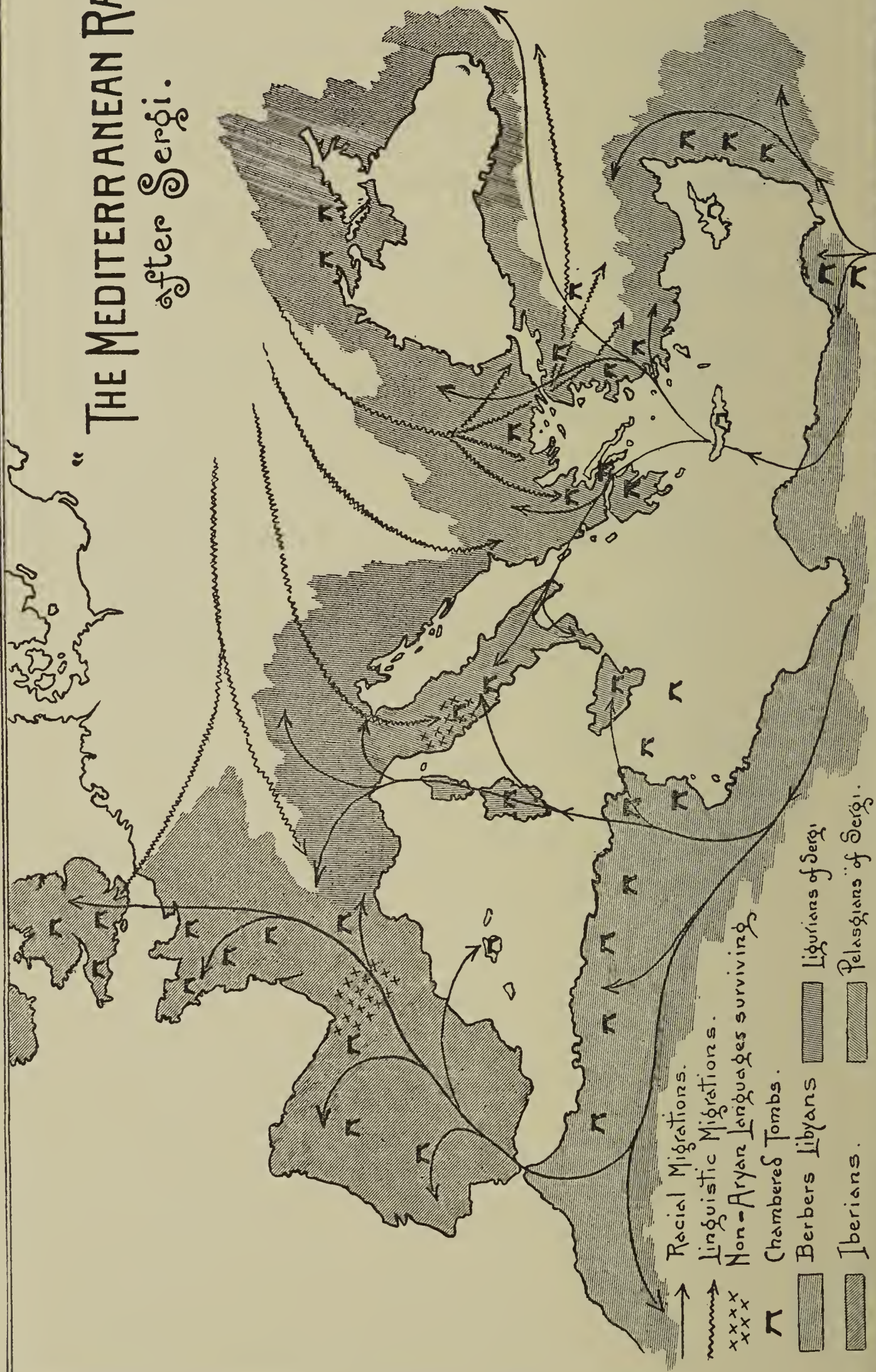
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JOHN E. MARR.

"THE MEDITERRANEAN RACE." after Sergi.



PREHISTORIC MAN IN THE EASTERN MEDITERRANEAN.

PART III.

MEDITERRANEAN ETHNOLOGY, ANCIENT AND MODERN.

THE last preceding section of this discussion closed with the question how far recent anthropological and archæological inquiries affect the validity of the Hellenic tradition, and of certain mainly philological conclusions which were commonly accepted until recently as fixing the ethnological position in early times of the inhabitants of the coasts of the Ægean and of the Eastern Mediterranean in general.

68. The Greeks themselves seem to have elaborated already in the sixth and fifth centuries B.C. a rationalised and on the whole consistent theory of their own origin, from what data we can only indicate in outline. In an explicit summary, Herodotus distinguishes four criteria of nationality: common descent, common language, common religious belief and ritual, and a common mode of life in things secular. Of the use of all these classes, examples are abundant already in his own pages, in those of Thukydides, and in surviving fragments of the rationalistic historians of the fourth century B.C.

69. But it must be remembered that to a Greek historian "community of descent" meant similarity of *traditions* of descent, unverified either by contemporary documents, or by more than the most superficial comparison of physical types: "community of language" was determined by equally superficial resemblances of individual words, traced without knowledge of phonetics, and in many cases without a working acquaintance with the non-Hellenic languages in question, or even with the remoter dialects of Greek; "community of religion," and "community of modes of life," seldom presupposed more than a certain similarity of non-essential names and forms, or such broad identity of funda-

mental purpose, as would prove nothing even between races which were really related to each other. With these drawbacks it is only to be expected that Hellenic anthropology should be inconclusive and inconsistent in detail ; but for the same reason it will be the more noteworthy if we find that its main outlines still prove serviceable as a working hypothesis.

70. One cardinal belief in particular could hardly have passed into common acceptance if it had not been founded upon appreciable fact. The Greeks of the classical period firmly believed themselves to be a mixed race, and held further that each of the primary components of the mixture was itself composite ; and variously composed in different districts. Stripped of its mythical and personal presentation, their view in outline was that a little leaven of a race of superior natural endowments, to which the name of Hellene more peculiarly belongs, had descended from South Thessaly, if not from Macedon, and had worked among a great mass of non-Hellenic barbarians, until the whole was leavened with Hellenic culture. Herodotus affirms distinctly that the inhabitants of Attica, and of some other districts, were not originally Hellenes, and had become so by acquiring Hellenic language and institutions. Thukydides adds that the superior race need never have been many in number, and that they owed their influence to the superiority of their civilisation, and not to any appreciable displacement of population.

71. The aboriginal pre-Hellenic stock passed under many names, among which "Pelasgian," the commonest and least vaguely conceived, passed in some measure into generic use. Prehistoric forts and other remains, mysterious cults and grotesque customs, archaic words of uncertain meaning, were explained as relics of "Pelasgian" barbarism ; some Greek-speaking tribes in a backward stage of culture were thought to be still imperfectly "Hellenised" ; and other remnants of earlier stocks lingering on either side of the Ægean highlands or on capes and islands, and still speaking a language which could not be recognised as Hellenic, were regarded as actual survivals of "Pelasgian," "Helegian," or "Minyan" peoples. A variety of evidence points

to a belief that the old race was of dark complexion ; to an early identification of its representatives in Greece and the Ægean with the earliest stratum of population in Italy and Sicily ; and to a hazy attempt to express a relationship vaguely felt to exist between these autochthonous peoples of Greece and Italy and the inhabitants of Libya, Egypt, and the Syrian coast.

72. In the strongest contrast with the autochthonous "Pelasgians," and the Ionian Greeks who seem to have been regarded as most closely akin to them, stand the immigrant Hellenes, whom Herodotus regarded as most purely represented in the Dorians, though reasons have lately been shown for holding that the Achæans had at least as good a claim to the title. Their progress was from the North, and in Peloponnese at least the last of them to arrive were never fully naturalised among the older race. Their ideal of beauty was fair and ruddy, and this type continued to assert itself in Hellenic art, side by side with the brunette type of the aborigines, at least as late as the close of the fifth century B.C., though in Græco-Roman painting it has already become rare ; and in Byzantine and Romaic art, as well as among the modern Greeks, it practically dies out altogether.

73. The stories of immigrations over-sea, from Asia Minor, Phœnicia and Egypt, are not in all probability to be regarded as analogous with that of the coming of the Northern Hellenes. They invariably refer either to individual adventurers, such as Danaos or Pelops, or, as in the case of the Lybian Kyklopes, to wonder-working craftsmen summoned for a specific purpose, and are clearly attempts to explain the introduction of what appeared to be foreign elements in the prehistoric civilisation of the Ægeans, by connecting them with the arrival of semi-mythical personages.

74. Until the first quarter of the present century this traditional account passed practically unchallenged ; and in most quarters survives in all its principal features, though hardly a single detailed statement has escaped critical modification.

75. During the second and third quarters of the century the principal criticisms which have passed upon it have

been based on philological evidence. The similarities between the so-called Aryan languages, from India to Wales, have been held to indicate as their common place of origin an area which is certainly not farther east than what is now Turkestan, nor farther west than what are now Scandinavia and Bohemia, and which within these sufficiently wide limits has been more exactly placed, with much probability, in the wide and fairly uniform plains of South Russia, between the Carpathian Mountains and the Caspian Sea. From this place of origin Aryan speech is inferred to have spread south-eastward to India, north-westward as far as Scandinavia and the British Islands, and south-westward into Central and Southern Europe.

76. From similarity of language has then been inferred community of blood, and a vigorous and rapidly developing Aryan race has been depicted, propagating its linguistic creed, and pressing before it aborigines, who for want of a better name have been simply described as pre-Aryans, and credited, until recently, with little or no civilisation of their own. In Greece, in particular, the migratory Hellene has been held to represent that branch of the Aryan race which broke through from the Danube Valley into the Balkan Peninsula; and Hellenic civilisation has been described in general terms as approximately Aryan in type.

77. The argument from similarity of language to community of race is, however, obviously a weak one, even when supported with secondary philological arguments from a certain community of religious ideas, mode of life, and type of civilisation among the various Aryan-spreading clans before their presumed separation. It seems an obvious remark that a language can be learnt, while physical structure cannot, and that as the widely different races which now speak Aryan languages cannot be regarded as varieties which have developed since the "migration," some of them at all events must represent non-Aryan and pre-Aryan races, who have acquired Aryan speech from the newcomers.

78. The obvious reply to this objection—that however the others may have acquired their languages, nevertheless some *one* of these distinct races must represent the original

Aryan-speaking stock—only challenges the further question: *Which*, then, is the Aryan stock? which are the non-Aryan recipients of Aryan speech? It would be disproportionate here, and it is fortunately unnecessary, to discuss the numerous attempts which have been made to answer this question. For of all the physical types which compose the population of Europe in historical and modern times, every one, the Turk only excepted, has been claimed as representing the original Aryan stock, or at least as closely akin to it.

79. We are therefore justified in putting all primarily philological hypotheses on one side; if only on the ground that none of them meet the more probable case, that the race, with which Aryan speech originated, may have been at all times few in numbers, and may further have been long since extinct; and consequently that *all* the Aryan speaking races of historic times may have, at one time or another, learnt Aryan speech, without acquiring more than the slightest tincture of Aryan blood. The ground is thus left open for a review of the whole question from a point of view primarily anthropological, and based in the first place on physical, namely morphological criteria of natural kinship between the races to be examined.

80. This is peculiarly important in the southern part of Aryan-speaking Europe, and this for two reasons. On the one hand we are here, on the philological theory, farther removed from the "Aryan Home," and separated from it by great natural barriers. The theory of wholesale migration, therefore, may here be examined in a crucial instance; and as a matter of fact the physical evidence actually gives a sufficiently coherent answer. On the other hand, it is exactly in these southern areas, which project into the Mediterranean, that the first great "Aryan" civilisations came into being, namely those of Greece and Rome which have in many respects so largely coloured current conceptions of the probable complexion of primitive "Aryan civilisation". It is therefore here that material and circumstantial evidence can be best brought to bear upon the outline of "Aryan culture," as inferred from philological

data. These civilisations, however, when examined on their material side, are found to present many features which it is impossible consistently to recognise as "Aryan". Further, in the very areas in which we find them historically, they seem to follow an essentially continuous series of development out of a thoroughly primitive and uncivilised stage, which is quite as unlike "Aryan civilisation" as is the specific culture in which they culminate. In fact, to anticipate for a moment a summary of the result, in an extract from Dr. Sergi's recent essay on the Mediterranean Race :—

Only a few years ago, the Greeks and the Romans were thought to be actual Aryans ; and after that they were thought to have been at all events completely Aryanised. But the great discoveries which have recently been made in the Mediterranean have overturned all these theories. To-day, in spite of the fact that they became at a late stage champions of an Aryan speaking culture, the conviction is forced upon us that the oldest civilisation of the Mediterranean is not of Aryan origin, but is the product of a race composed of many blood-related peoples, who have come from a common starting-point, though in the Mediterranean area they pass under different racial names.

81. These two lines of evidence, physical and cultural, anthropological and archæological, must, however, be kept as clearly distinct from one another as from the philological evidence ; for it is in theory at all events, as easy to *learn* a mode of burial, of worship, or of metal working, as to acquire a language. Culture that is like language, taken by itself, proves, and can prove, nothing directly about Race. Like linguistic evidence, however, cultural evidence may justify important confirmatory inferences in support of a hypothesis based primarily on the morphological data.

82. The conclusions which seem to be fairly deducible from the extant remains of the first known civilisation in the Eastern Mediterranean, as to its indigenous character ; the course of its growth ; its wide influence upon the first civilisations of the Western Mediterranean, and of Central and Western Europe ; and its essential continuity, through temporary and partial eclipse, with the "Hellenic" civilisation of historic Greece, were outlined in the first paper of this series ; while the second summarised the course of recent

speculation with regard to the probable authors of this "Ægean" system of culture. It only remains, therefore, to indicate, equally briefly, the present state of our knowledge of the ethnography of the Mediterranean from the time when it first becomes traceable.

83. The Mediterranean basin, as we know it, results from the reunion of at least three detached basins by the resubmergence of the land barriers which had joined Europe and Africa in pliocene times. An approximate date for their submergence may be inferred from that of the isolation and rapid disappearance of characteristic African fauna on the north side of the reunited basin. The human occupation of the same north or European side may have been similarly interrupted by the same climatic changes which extinguished the African fauna in the same area.

84. But exceptions to this theory of a general separation of Palæolithic from Neolithic man on European sites have been recently supplied by deposits of the Baoussé Roussé caves in the Riviera, which were at first taken to be of early Neolithic date, but have since been recognised as strictly transitional; the human remains being of Neolithic type; the associated objects still quite Palæolithic in character.

85. On the south side of the Mediterranean the evidence is at present fragmentary, and derived from areas which are not yet half explored; but both a late Palæolithic and a very early matured Neolithic civilisation are already indicated at a number of points. The geographical circumstances also indicate that there existed formerly in North Africa a very much wider area of habitable country than now, and a comparatively favourable climate for a long though not exactly measurable period.

86. The geographical and archæological hints are amply borne out by the morphological data. From Somali-land and Abyssinia, through Egypt, Libya, and Mauretania, to the Canaries, the fundamental type of the native population, ancient and modern, is from east to west practically identical, though from north to south a negroid taint is increasingly perceptible, and though, besides this, marked varieties of complexion and facial feature have been re-

peatedly recorded both in Egypt from the time of the earliest portrait records (c. 3000 B.C.) and in what is now Tunis and Tripoli from the time of Herodotus onwards.

87. It has been long recognised that the predominant and longest established element in the population of Spain, namely, the type called Iberian, is very closely related to the Mauretanian, Berber, or Libyan type of the opposite African shore: and that the similar brunette and dolichocephalic element which pervades the population of the West of France and of our own country is to be regarded as a further extension of the same immigration from the South.

88. In Sicily and South Italy, a similar overflow of Libyan peoples seems to be indicated by the predominance of a type almost indistinguishable from that of the Spanish area, and already recognised in the fifth century B.C. by Thukydides or his authorities, as closely akin to it; a fact which confirms the impression that the Arab element, which here, as in Spain, has to some extent to be taken into account, is by no means wholly responsible for the frequency of North African analogies. In Italy, however, at all events, the peculiar local modifications of this type, or group of types, which occur, are in the direction of similarity to varieties which are so characteristic of the Greek peninsula and islands, that they cannot be wholly attributed to the continuous intercourse which has gone on across the lower Adriatic since the beginning of historic time.

89. The question then arises: May we infer an overflow of North African peoples into the Ægean area, similar to that which has been already noticed from Morocco into Spain and from Tunis into Sicily, to have occurred at any early period between a similar, though, it is true, less closely related pair of land prominences, namely, the Cyrennica on the south and Peloponnese prolonged through Lytheia into Crete on the north?

90. In Greece and the Ægean, unfortunately, the discoveries of human remains of early date have hitherto been comparatively rare; but the predominant types which are indicated by the published evidence correspond here also very closely with the same group of North African "Hamitic"

types which have been described already. It is true that even as early as the "Second City" at Hissarlik, one of the skulls found by Dr. Schliemann presented close resemblances to the Thracian type of Central Europe; but it may be taken as very probable that this and the rare parallel instances only represent the first beginnings of a progressive infiltration of northern brachycephalic races from beyond the mountain barrier, which has succeeded in modifying slightly the modern Greek type by an increase for example of the cephalic index from about 75 to 77; but, as in the case of the Lombards and Cormans in Italy, has hardly affected the characteristic outward type of complexion, eyes and hair.

91. East of the Ægean, the evidence for the earlier elements of the population is even more fragmentary, and at the same time the disturbances which have resulted from the Mongolian inroads of the Middle Ages are more marked. But the observations of Von Luschan and Benndorf have demonstrated a general correspondence with the same Mediterranean type; and an increasing series of skulls from Cyprus confirm that conclusion.

92. At this corner of the Levant however the whole question is complicated by the proximity and very early intrusion of races from the Syrian coast land, of the well-marked type whose distribution seems closely to correspond with the primary area of Semitic speech. This type however, as Prof. Flinders Petrie's recent observations show, is itself so closely allied to the Hamitic types of North Africa as to be difficult to distinguish from it in contiguous areas, and presents a number of intermediate forms which are probably actually half-caste.

93. Thus a survey of the whole of the Mediterranean coast-land leads to the conclusion that its earliest recognisable inhabitants and their descendants, who form the great mass of the present population, belong to a single closely connected group of races; that their earlier home is to be looked for in the formerly fertile interior of North Africa, and not improbably, as Dr. Sergi has indicated, in or near the upper valley of the Nile; and that the peninsulæ of

South Europe and Asia Minor have been peopled thence along several district routes which mainly follow the course of the pliocene land-bridges.

94. The "Mediterranean Race," thus described, has the following characteristics common to all its branches: The outer complexion is typically brown; brown skin; brown eyes, brown hair, abundant, and always more or less wavy. It is thus equally distinct from the blonde white races which bound it on the north, and from the negro races of Africa. Modifications of the brown tint are found in all branches of the race; but are conceived to be due to intermixture either with yet earlier aborigines or with subsequent intruders. The body is well proportioned, the face oval, the nose rather narrow, the orbits wide and set horizontally, the forehead high and nearly vertical, the cheekbones neither wide nor very high; the face not flattened, but if anything a little prominent in front; the neck long and well rounded, and the features mobile and expressive. It is in fact the familiar brunette type which every one recognises who has travelled in any part of the Mediterranean.

95. The forms of the skull are more variable, and have been somewhat differently interpreted by a number of investigators. To Dr. E. Sergi, however, is due a suggestion which at the same time explains the prevalence of a number of concurrent types of structure over so wide an area, and relieves us from the necessity of attributing so great importance to their divergences as has sometimes been the case. He rejects, except as a convenient *memoria technica*, the traditional and orthodox method of cranial measurement (to which he refers rather scornfully as the "anthropology of the indices") on the ground that the length and breadth measurements usually taken express merely resultants of groups of growth tendencies on the part of the various bones which compose the skull, and that such resultants may—as is obvious—be composed in a variety of ways. And certainly to classify mankind merely by the ratio of the length to the breadth of their heads, or by any other such arbitrary ratio, is little better than it would be to classify animals in general by the ratio of the length to the breadth of their whole body;

a method which would occasionally produce a surprising redistribution of affinities. Indices, it is true, are like finger-marks, an admirably compact summary of individual characteristics ; but without full morphological commentary they may give very inadequate definitions of a species or variety.

96. For these empirical measurements Dr. Sergi substitutes a classification into morphological types, according to the general form of the skull. The method, in fact, is practically that of compound photography, and the principal types bear merely descriptive names such as illiptical, pentagonal, rhomboidal, or egg-shaped ; qualified by specific names, either descriptive, racial, or geographical. Determined by these tests, the Mediterranean Race appears, wherever it is found, as a collocation, more or less uniformly complete, of a number of such related types : and from this it is inferred that the Race was already composite in the farthest area of origin to which it can be traced.

97. This centre is placed in Dr. Sergi's map, and, as already indicated, in the Upper Valley of the Nile, on the ground that here, among the Abyssinians, Gallas and Somalis the characteristic collocation of types is most completely exhibited ; the dusky complexion of a large proportion of these races at the present day being discounted, partly by their long-continued exposure to a more tropical climate than any other branch of the race ; partly by the certainty of continuous infusion of a Negroid strain from the south. It is also on this hypothesis possible to explain the very close likeness between the Eastern Hamitic and the Semetic types, and the ambiguous position and composite character of the Egyptian nationality between them ; for a migration seawards down the Nile must necessarily divide at the Delta into two streams ; and of these one must then move westwards along the Libyan coast, and the line of the oases, formerly much larger, which lie behind it, while the other must move eastwards into North Arabia and South Palestine ; where Prof. Flinders Petrie has shown that the primitive Amorite population exhibited a physical type, and a fairly advanced civilisation almost indistinguishable from that of the Libyan element in pre-Dynastic Egypt.

98. Archæological evidence confirming this original connection of the peoples of the Mediterranean basin seems to be afforded by the extension over approximately the same area and from Neolithic times onwards of the custom of burial in sepulchral chambers, either rock-hewn or constructed on or close below a level surface ; and in the latter case covered by a mound of earth or stones. As typical examples of this widespread type of interment, and in illustration of the diverse local developments which it has undergone, we may cite alike the "mastabas" and Pyramids of Egypt, the dolmens and chambered tumuli of Tunis and Algeria, of Spain, the West of France, and our own islands, the Nura-ghe of Sardinia, the "tombe a camera" of Etruria, the "bee-hive" tombs of Greece and the Ægean, the chambered tumuli of Karia, Lydia, and Phrygia, and perhaps also, according to Dr. Sergi, the Kurgani of Southern Russia. For these monuments not only range over approximately the same area as the race in question, and accompany the development of a primarily homogeneous civilisation, but are always found to be tenanted by representatives of the same physical type, wherever their contents are sufficiently well preserved.

99. In the same way, a number of independent investigations of the "pre-Aryan" languages which survive into historic times within the same area seem to converge upon the conclusion that the Mediterranean basin corresponds also to an early linguistic province.

100. Meanwhile, alongside of this whole group of inquiries tending to establish the essential unity, and independent native development of the Mediterranean province, a similar series of conclusions are taking shape, which bear directly upon the second component recognised in the traditional scheme, with which Ægean and eventually all Mediterranean ethnology historically begins ; and with an almost identical influence upon that early hypothesis. In this restricted and qualified field the Aryan hypothesis has proved a valuable working suggestion, especially since it became probable that not merely the Italic and Hellenic groups of languages were of kindred northern origin, but that the latter was intimately related with the extinct

language of Thrace as an intermediary to a group of languages certainly intrusive but long dominant in Asia Minor ; of which Phrygian and Armenian are the best preserved examples.

101. Moreover on this side of the Ægean also the linguistic invasion coincides with an even better preserved tradition of a recent but already evanescent overflow of highly endowed and politically dominant clans from South-east Europe into the Anatolian coastland, and even on to the Phrygian plateau. More than this, the overflow in question was not yet at an end even at the opening of Hellenic history ; in the Homeric " Catalogue of the Allies of Priam," a document which in every other particular runs in correct geographical order, Bithynia, which is practically Thrace-in-Asia is significantly omitted : and the Kimmerian invasion of Asia Minor in the seventh century B.C. can only be satisfactorily interpreted as originating, like the Bithynian invasion, and probably in the closest connection with it, from South-east Europe. The inroad of the Gauls in the third century B.C., which resulted in the superposition of Galatia upon the south-east part of immigrant Phrygia, is of course an almost exact repetition of the same series of events.

102. Archæological evidence also occurs in the same sense, though it is fragmentary and for the most part still much disputed. That the culture province of the Danubian basin, from the first moment of the trade in tin and amber across the mountain barrier, exercised an appreciable reflex influence upon the civilisation of the Ægean area, has been now for some years undisputed ; and it is highly probable that, as in the later examples of Greece, of Rome, of provincial Gaul, and, as Mr. Evans has suggested, of Celtic Ireland, this commercial intercourse first revealed to the waking intelligence and restless energy of the peoples beyond the Balkans, as afterwards beyond the Alps and the Rhine, the resources and amenities of the coastlands of the Mediterranean ; whereas on the Aryan paradox, *E Borea lux*, no motive is supplied for these southward immigrations into lands which *ex hypothesi* must have been still barbarous. On this side the recent papers of M. Salomon Reinach,

Mr. Evans and Prof. Ridgeway summarise fully the present acutely transitional character of current views on the ethnology and early civilisation of the Ægean, the two key-notes of which, as has been indicated more than once in the preceding paragraphs, are the insistence on the original and independent character of the Mediterranean province, and particularly of the Ægean area of it; and with regard to the admitted subsequent intrusion of ethnic, linguistic and cultural elements from the north, a return to a general presentment of the evidence, which almost literally coincides with that of the Hellenic anthropology of the sixth and fifth centuries B.C.

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THE EXTRACTION OF GOLD AND THE CYANIDE PROCESS.

PROGRESS in the metallurgy of gold has of late undergone a complete change. The empirical stage of the art persisted long after the importance of the scientific foundation underlying most other industries had been fully recognised. The victory of common sense over ignorance or prejudice has been a slow one, and for many years the "practical" man continued to scorn his "theoretical" contemporary, until at length the two became united in the scientific metallurgists who are to-day engaged in all parts of the world in the production and purification of the precious metals. Research work, under conditions likely to lead to useful results, and conducted by men who are familiar with the problems to be solved, has been rendered possible by the huge scale on which operations are now conducted in many parts of the world. Parties of diggers working surface deposits have no time or money to investigate knotty points in the metallurgy of gold, but it is otherwise with the great companies which deal with the South African reefs, and the rate of progress in scientific metallurgy has been greatly increased by the means which these wealthy corporations have been able to provide. Moreover when chemistry makes a present of some useful fact to her technical sister, a whole army of workers now fall upon it, dissect it, amplify it, and soon enrich pure science with many return gifts.

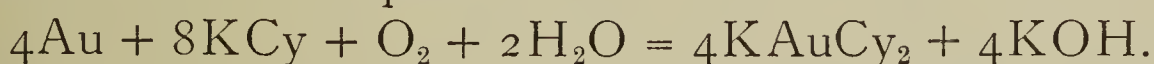
The way in which scientific discoveries and their technical applications may react on each other is well exemplified in the history of the work which has been done on the solvent action of potassium cyanide on gold. Dr. Wright of Birmingham discovered this action in 1840, and it was mentioned in a patent specification relating to electroplating taken out by Elkington in the same year. But though Bagration¹ in 1843, Elsner² in 1846, and

¹ *Bull. de l'Acad. des Sciences de St. Petersbourg* (1843), vol. ii., p. 136.

² *Erdm. Journ. Prak. Chem.*, vol. xxxvii. (1846), pp. 441-446.

Faraday¹ in 1857 made long series of experiments on the subject, the discovery remained for nearly half a century one of the unused and apparently useless chemical data which help to fill text-books for a time and are then omitted as of no interest and finally almost forgotten. When it was found, as the result of repeated trials in 1886, that alkaline cyanides in dilute solution are fairly stable substances, their solvent action on gold became of value to mankind, and since then an ever-increasing army of workers has been carefully experimenting on the action of cyanide not only on gold but on other metals, on sulphides, oxides, and silicates, on wood and a hundred other substances, with the result that the data accumulated would fill a volume by themselves.

Much light has been thrown, for example, on the exact mechanism of the chemical change which ensues when cyanides act on gold. Elsner found that the air at the top of an inverted test-tube containing gold dipping into a solution of cyanide of potassium had lost its oxygen after twenty-four hours, and considered that this had been consumed in dissolving the gold, although the oxidation of cyanide to cyanate might have accounted for its disappearance. Faraday discovered that if gold leaf is floated on the surface of a solution of cyanide it is dissolved many times more quickly than if it is completely immersed and so protected from the air. It was subsequently proved by Mac-laurin² that pure gold is not soluble in a solution of pure cyanide if oxygen is completely excluded, and that dissolution is greatly increased if the liquid is thoroughly aërated and especially if oxygen is continually bubbled through it. Much evidence was afforded by him in support of the correctness of the equation:—



Nevertheless this equation does not represent the whole of the chemical action, as a substance reacting like hydroxyl seems to be produced. To explain this, G. Bodländer of Clausthal puts forward the equation:—

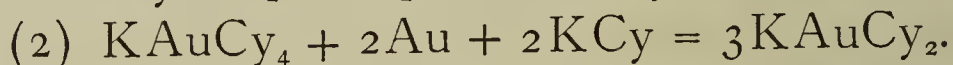
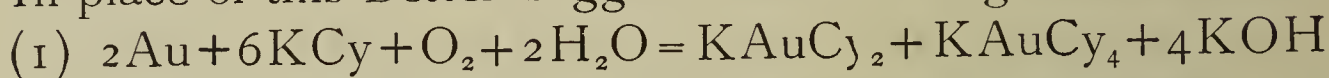
¹ *Roy. Inst. Proc.*, vol. ii., p. 308.

² *Jour. Chem. Soc.* (1893), vol. 63, p. 724.

(1) $2\text{Au} + 4\text{KCy} + 2\text{H}_2\text{O} + \text{O}_2 = 2\text{KAuCy}_2 + 2\text{KOH} + \text{H}_2\text{O}_2$
 followed by—

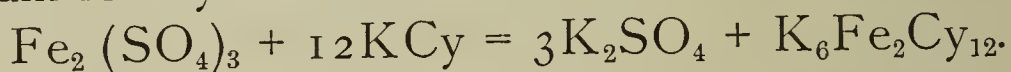


In place of this Bettel¹ suggests the following :—



Even if such actions occur, however, it is possible that they are limited to an insignificant part of the whole mass.

However this may be, it is fully proved that gold cannot directly displace potassium or sodium in alkaline cyanides, the liberation of hydrogen in the liquid never having been observed, whilst conversely the displacement of gold by metallic potassium is readily effected, and is complete. It is necessary that some substance should be present having a strong affinity for potassium, so as to unite with it, in order that gold may be dissolved by cyanide. The usual agent employed is oxygen, but it is not necessary that it should be in the free state, many substances containing it loosely combined being efficient substitutes. Mac Arthur, indeed, has cited experiments² to show that gold in ores can be dissolved by potassium cyanide in the absence of oxygen, and Bettel³ found this to be the case, if the crushed ore contains basic ferric sulphate (a common constituent where oxidised pyrites are present), by which potassium ferricyanide is formed, thus :—



The addition of other oxidising agents to expedite the action of cyanide has been suggested on all sides, and the results of work in this direction have been the basis of several variations in the original process. Some of the most interesting results are those obtained by Bettel and Marais in 1894.⁴ They first removed all solvent power from a cyanide solution by expelling the dissolved oxygen with a current of hydrogen, and then added various oxidising agents and observed the effects produced by them. Under these circumstances, neither potassium bichromate,

¹ *South African Mining Journal*, 8th May, 1897.

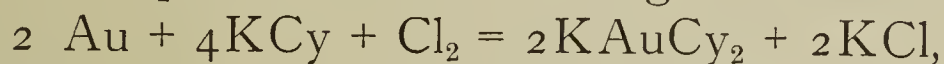
² *Jour. Soc. Chem. Ind.* (1890), p. 7.

³ *Loc. cit. ant.*

⁴ *Proc. Chem. and Met. Soc. of S. Africa*, May, 1897.

chromate, chlorate, perchlorate, nitrate nor nitrite enabled the solution to dissolve gold leaf, and ferric hydrate and bleaching powder were also without effect. The addition of pyrolusite gave a doubtful result, and lead dioxide caused very slow dissolution of the gold. Doubtless the presence of hydrogen tended to hinder these substances from assisting the cyanide, but under similar conditions if ferric chloride, chlorine, or iodine dissolved in potassium iodide were added, gold was dissolved slowly. The action was more decided if an addition was made of potassium ferricyanide or permanganate, sodium dioxide, hydrogen peroxide or barium dioxide. Finally, gold dissolved rapidly when bromine was added.

It is probable that ferric chloride and the halogens act without any intervention of oxygen, the superfluous potassium being converted directly into a haloid salt according to some such equation as the following :—



some cyanide of chlorine being formed in the portions of the solution where gold is not present.

The essential point is that some substance, having a strong affinity for potassium, must be present to assist in the displacement of that element by gold in the cyanide molecule. Whether the removal of the potassium is effected by oxygen or a halogen is immaterial if the mass or concentration of one of these agents is sufficient, and although their minimum effective concentration is unknown it is clear that, as cyanide is not a solvent without the aid of one of them, it is useless to increase the strength of cyanide without a corresponding increase in the amount of the oxidiser. This is the reason why strong solutions of cyanide are not better solvents of gold than weak ones, unless means are taken to increase the quantity of available oxygen. Moreover, since cyanide acts directly on the sulphides of the base metals (which are usually present in gold ores), without requiring the assistance of oxygen, a greater waste of the solvent results in proportion as the solution is stronger. The reason for the necessity of circulating the solution in treating ores is also clear, as the very

small quantity of oxygen (amounting to only about 0·4 per cent. by volume in good solutions in the Transvaal) in the neighbourhood of a particle of gold would soon be exhausted, long before the cyanide could be saturated with gold.

It is not necessary to take special means to add oxygen to cyanide solutions when ores, such as Transvaal tailings, poor in gold and free from reducing agents, are in course of treatment. Enough air is entangled in the ore or dissolved in the solution for all practical purposes. When pyritic ores are treated the supply of oxygen is exhausted before the whole of the gold is dissolved, and it has been found desirable to resort to the "double treatment," as for example at the Primrose Works at Johannesburg, where pyritic tailings are leached in two vats in succession, the process of draining dry and transferring the ore being chiefly beneficial on account of the aëration that is thus effected. When concentrates rich in sulphides came to be treated, the difficulties of supplying sufficient oxygen were found to be still greater. The gold dissolves with such extreme slowness that treatment occupied two or three weeks, even if the ore was drained dry at intervals and stirred up.

It has long been remarked that a small percentage of a soluble sulphide present in the cyanide solution greatly delays the dissolution of gold. Doubtless this is partly owing to the abstraction of oxygen from the solution by the sulphide, for gold sulphide is freely soluble in KCy so that the surface of the metal is kept free from sulphide if the cyanide is not too dilute. Bettel however points out¹ that silver sulphide is far less soluble than gold sulphide, and that if native gold is alloyed with 20 per cent. of silver, no uncommon occurrence, a film almost insoluble in dilute cyanide solutions may be formed. It is certain that some specimens of gold leaf dissolve with great difficulty if they have been previously dipped in sulphide solutions, or if traces of soluble sulphides or sulpho-cyanides are present in the solution. The difficulty disappears if the sulphides are removed, either by being precipitated with lead salts, or by the action of certain oxidisers.

¹ *South African Mining Journal*, 8th May, 1897.

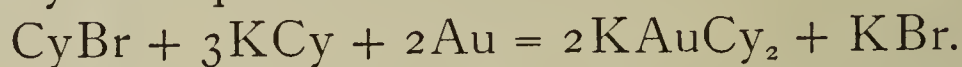
The limit of difficulty of dissolution is reached when the ore contains some rapid absorbent of oxygen such as ferrous sulphide, when, instead of encountering delay the operator finds that the gold is not dissolved at all. In such a case it is necessary to saturate the substance completely with an excess of oxygen before applying the cyanide solution. It is in this way that Caldecott solved the problem of getting into solution the gold contained in accumulated slimes on the Rand.¹ The gold in slimes fresh from the battery is readily soluble in cyanide solution, being in a very fine state of subdivision, but it is quite otherwise with slimes which have accumulated in dams and settling pits and have been exposed to the weather for some time. Under these conditions the iron pyrites is rapidly decomposed into ferrous sulphide and free sulphur, the impervious nature of the materials preventing free access of air which would result in the formation of sulphates and free acid. To prepare these slimes for treatment Caldecott supplies oxygen artificially in the form of air delivered from a perforated pipe fixed near the bottom of the agitation vat containing the pulp. Aeration of the slimes has now been regularly used since the end of 1896 at the Rand Central Ore Reduction Company's Works, accelerated when this operation takes too long or when much organic matter is present with from two to eight ounces of permanganate of potash per ton of dry slimes. After the presence of ferrous sulphide can no longer be detected in the slimes, the aëration is still carried on for an hour or more, in order to oxidise any ferrous hydrate remaining and cyanide is then added, the gold being now found to be readily soluble.

Such treatment represents what is necessary in an extreme case, and the addition of potassium permanganate or sodium dioxide is often made in the United States and elsewhere merely to increase the rate of action of cyanide. In the Sulman Teed process, now in operation on arsenical pyritic ore at Deloro in Canada,² oxygen is dispensed with

¹ *Proc. of the Chem. and Met. Soc. of S. Africa*, 17th July, 1897.

² See paper by Hugh K. Picard read at the London meeting of the Federated Institution of Mining Engineers, May, 1898.

and cyanide of bromine added, when potassium bromide instead of the hydrate is produced, the action being expressed by the equation



As already observed, the same direct removal of potassium is probably effected when the halogens are added to cyanide solutions. But, on the other hand, Clennel has advanced some evidence¹ of the formation of considerable quantities of hydrocyanic acid, when bromide of cyanogen is added to potassium cyanide, and a corresponding amount of oxygen would thus be rendered available either to form cyanates or potash, so that the action of bromine would after all be merely one of oxidation.

Although the alkali metals are positive to gold in cyanide solutions there are others which can be directly displaced by both gold and silver. Mercury is one of these, the solvent action of the double cyanide of potassium and mercury being independent of the presence of any substance having an affinity for potassium, and this double salt is sometimes used on refractory ores in which the gold is difficult to get into solution. Other elements, such as iron, lead and carbon, which are negative to gold in cyanide solutions, also increase the rate at which it is dissolved if in contact with it, but the difficulty of obtaining contact is too great for these substances to be of any practical value.

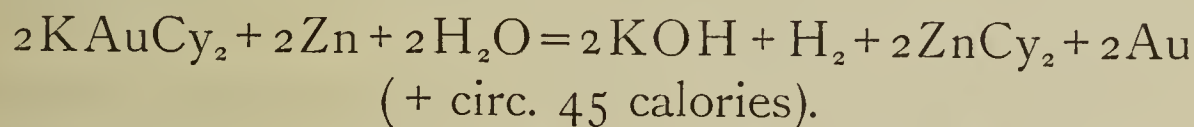
In general the various quickeners of the action of cyanide are unnecessary with simple ores, as for example in the Transvaal, where the leaching of the tailings occupies two or three days owing to mechanical difficulties, so that nothing is gained by reducing by a few hours the time necessary to dissolve the gold. On the other hand an abundant supply of oxygen in almost any form increases the destruction of the cyanide in various ways, so that the cost may become almost prohibitive.

The consumption of cyanide has been undergoing reduction by repeated steps during the last few years by means of decrease in the strength of the solution. Beginning

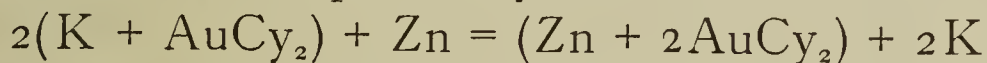
¹ *Proc. Chem. and Met. Soc. of S. Africa*, vol. i., p. 127.

with 1 per cent. solutions the metallurgists of the Rand soon cut this down to 0·3 or 0·4 per cent., and the limit seems now to have been reached in the treatment of slimes, in which solutions containing 0·001 per cent. are in use, or only one-third of an ounce of cyanide in a ton of water.

The question of dilution is intimately bound up with that of precipitation. In the early days when the zinc process only was practised, it was found that the gold was precipitated more rapidly and completely when a considerable excess of free cyanide was present, and it was common to make up the strength of the solution coming from the leaching vats before passing it through the zinc boxes. In particular, pure sheet zinc is quite unable to precipitate gold in the absence of free potassium cyanide, although the fili-form zinc, consisting of thin threads, often as much as a yard long and $\frac{1}{160}$ th inch wide, slowly extracts gold from the same solution.¹ Christy suggests² that the cause of this is that hydrogen is set free by the first action of the aurocyanide of potassium on the zinc, thus:—



This equation is equivalent to stating that the K ions in the aurocyanide are replaced by zinc, as follows:—



followed by a rearrangement of the molecule of zinc-gold-cyanide, and the replacement of the gold by another atom of zinc. This mechanism of change seems the more probable when it is remembered that the K ions in KCy itself are displaced by zinc in the presence of water, and that the whole reaction is more strongly exothermic than that of simple displacement of gold by zinc.

Assuming, then, that hydrogen and zinc cyanide are set free as above, imperceptible layers of both would be formed on the surface of the sheet zinc, which would thus be soon protected from further action. Hydrogen would also be set free on the surface of the thread zinc, but in this case the

¹ "The Precipitation of Gold by Zinc-Thread from Dilute and Foul Cyanide Solutions," by A. James, *Am. Inst. Mag. Eng.*, Feb., 1879.

² "The Solution and Precipitation of Gold," *ibid.*, 1896.

ragged edges of the shavings would assist the gas to form into bubbles and become detached, so that less "polarisation" would occur, but it must be admitted that the well-known evolution of hydrogen in the zinc boxes may be explained in other ways. The layer of ZnCy_2 is at once dissolved if potassium cyanide is present, and in its absence potash has a similar but less active effect, potassium zincate being formed.

The view, however, formed too hastily without sufficient experiment, that if the strength of free cyanide falls below about 0.2 per cent. precipitation is impracticable, has now been abandoned except by the advocates of rival precipitation processes. McBride obtained satisfactory precipitation by zinc at the Glencairn Mine¹ with solutions containing as little as 0.025 to 0.03 per cent. KC_y , and James showed the conditions to be observed in such cases, one of them being that the solution must be in contact with the zinc for at least an hour, so that 130 cubic feet of shavings (weighing 1 lb. per cubic foot) are required to treat 100 tons of solution in twenty-four hours.

It must, of course, be borne in mind that the zinc used is not pure, and that the small percentage of lead usually contained in it has a powerful effect in increasing its action, a couple being formed with the zinc as the positive element. The gold-zinc couple, produced after precipitation has proceeded for some time, is probably also of value, but on the other hand Goyder has shown² that the presence of iron in contact with the zinc is injurious, checking the rate of precipitation of the gold and increasing the waste of zinc and the tendency for the formation of ZnCy_2 on the threads. Iron screws should therefore not be used in the precipitation boxes. Ehrmann more recently³ tried the copper-zinc couple and found it most efficacious in hot solutions, but even in the cold his results were striking. For example, a cold solution containing 0.017 per cent. KC_y and seven dwts. of gold per ton had 61 per cent. of the latter pre-

¹ *Proc. of the Chem. and Met. Soc. of S. Africa*, vol. i., p. 289, 1897.

² *Chem. News*, vol. lxxiii. (1896), p. 272.

³ *Chem. and Met. Soc. of S Africa*, 17th April, 1897.

precipitated by zinc in twenty-four hours, and, under precisely similar conditions, 98·67 per cent. of the gold was precipitated by the copper-zinc couple, the residual liquor containing only two grains of gold per ton.

Apart from the zinc process, the only other method in extensive use of recovering the gold is that of Siemens and Halske, in which a current of electricity is passed from iron anodes to sheet-lead cathodes through the solution. The electrodes are placed from $1\frac{1}{2}$ to 3 inches apart and a current of four volts, and from 0·03 to 0·05 ampere is used. The gold is precipitated on the lead, which, after a run of one or two months, is removed, melted and cupelled. The chief advantage derived is that precipitation is possible from very dilute solutions containing from 0·001 to 0·1 per cent. KCy, and as these are almost, if not quite as effective as stronger solutions in dissolving gold from its ores, while suffering less loss from decomposition of the cyanide, some saving is effected by their use, estimated by Bettel to be about $\frac{1}{3}$ lb. of cyanide per ton of tailings. One of the main difficulties in electrical precipitation lies in the great resistance offered to the passage of the current by the dilute solutions employed. In order to reduce this as far as possible the electrodes must be of large size and the distance between them small, so that the use of mercury for the cathode, otherwise marked out as the most suitable substance, is not practicable.

The ions of aurocyanide of potassium are K and AuCy_2 . As the result of electrolysis, therefore, potassium is set free at the lead cathode, where it attacks the water forming potash and hydrogen; at the same time the gold in the double cyanide is displaced and precipitated, both by the potassium and the nascent hydrogen. Such hydrocyanic acid as may be formed is neutralised by the potash. Meanwhile the anion, AuCy_2 , is set free at the anode, but is at once split up into AuCy and Cy ; the latter unites with the iron, forming cyanides which become converted into Prussian blue, and are also oxidised in part, forming ferric oxide, and the cyanide of gold is partly precipitated in this substance, although if enough free potassium cyanide is present it may

be kept in solution. When precipitated the gold cyanide can afterwards be recovered by treatment with KCy. It is clear therefore that the solutions must be circulated, and also that the more free cyanide is present the greater is the percentage of gold precipitated on the cathode.

The value of electrical precipitation of gold from cyanide solutions has in no case been more clearly demonstrated than in the treatment of slimed ore, and it may fairly be claimed that the problem of its treatment, which for the past three years has taxed the ingenuity of the chemists and engineers of the Rand, could not have been solved without the aid of Siemens and Halske's process.

When the Rand ore is crushed in stamp batteries, part of it is reduced to an excessively fine state of division, which settles very slowly in still water, and if allowed to remain in the tailings prevents leaching in large vats from being carried on effectively by forming impervious layers in the sand. It was found necessary therefore in South Africa to separate the slimes from the rest of the tailings by running the pulp from the batteries into large vats full of water, when the coarser particles of ore sink and the slimes overflow at the top. The slimes amount to about 30 per cent. of the dry weight of the ore, and though their assay value is not high, nevertheless when thousands of tons of them were run to waste every day the aggregate loss was very great. The problem of the extraction of the gold from this material was therefore one of great importance. It was attacked by many able metallurgists in the Transvaal, and the economic success of the treatment now adopted as the result of their labours will have a considerable effect on the future prosperity of the gold-field. The method of treatment is as follows¹:—

The slimes as received from the mill are suspended in water, of which they form about $2\frac{1}{2}$ per cent. Milk of lime is added to coagulate the fine ore so as to assist it to settle, and part of the water is then got rid of by Spitzkasten followed by settlement in continuous-overflow pits. The settled slimes, containing about 50 per cent. of water, are

¹ Address by Chas. Butters, *Journ. Chem. and Met. Soc. of South Africa*, Feb., 1898.

then ready for cyaniding, and the superfluous water is pumped back for use again in the batteries.

The difficulties in further treatment have little to do with getting the gold into solution, but are due to the impossibility of passing liquids through the clayey mass. It is necessary to wash by decantation, and the amount of liquid thus obtained is from six to ten tons per ton of slimes, the solutions containing only from 0·01 to 0·001 per cent. of cyanide and, after the ore has been treated by them, from six to twenty-four grains of gold per ton, and the recovery of gold from these solutions was an entirely new problem for the metallurgists.

Several methods are in operation in the Transvaal for the solution of the gold, but the best is probably that in which the slimes together with cyanide solution are passed by a centrifugal pump from one tank to another. In this passage of the pulp through the pump and pipes, air is drawn in through the joints and glands, and is introduced into the suction of the centrifugal pump. In this way a large amount of oxygen is supplied to the pulp whilst it is undergoing the most perfect agitation in its passage through the pipes, and further aëration is effected when the stream falls into the vat, carrying air down with it. H. T. Durant was responsible for the final modifications in this system, which had been introduced by J. R. Williams, and the time required for the solution of the gold is now very short, being mainly effected during the passage of the pulp from one vat to the other.

It is found to be very undesirable to attempt to reduce the amount of solution added at one time to the slimes. In thick slimes, even lumps of cyanide become coated over and protected from the solvent action of the water, although rapidly dissolved if there are four or five tons of water to one ton of slimes, and the same effect is observed in dissolving gold. To enrich the solution, however, two charges of slimes are treated in succession by it before it is passed to the precipitation plant. After settling for twenty-four hours the solutions are decanted, and at the Rand Central Ore Reduction Company's works the slimes are finally transferred to vats 50 feet in diameter and 16 feet deep, holding nearly

1000 tons, where further settling takes place, and the slimes, discharged once a week, are found to retain only about 40 per cent. of moisture. The settling capacity of these vats has lately been increased by the delivery of the stream of pulp through a pipe of large diameter carried half-way to the bottom of the vat, so that the disturbance produced by additions is less marked and the settling proceeds without interruption. The settled slimes are discharged from these vats by a jet of water under 200 lb. pressure per square inch, which rapidly disintegrates the clayey mass, so that 500 tons weight of slimes can be discharged in three hours with little labour and at a low cost.

In the electrolytic precipitation, it is necessary to free the liquid from all solid suspended particles to prevent the electrodes from being coated with slime. This is done by filtering through sand or by prolonged settlement, but the clearness of the solution also depends on the solubility of the anode. By keeping the current density below 0.04 amperes per square foot, however, very little corrosion of the iron anodes takes place, a plate $\frac{3}{16}$ inch thick being estimated by Butters to last five years, while with Andreoli's peroxide of lead anode, practically no corrosion of the anode takes place at all.

Recent details of the exact cost of treatment of slimes are not available, but J. R. Williams gives it for the months of April and May, 1897, at the Crown Reef Mill as 3s. 9d. per ton including maintenance. Of this amount, cyanide cost $5\frac{1}{2}$ d., lime 6d., and royalty for the use of electrical precipitation nearly 5d. The Crown Reef Company, by means of its stamp mill, cyanide plant and slimes plant is now recovering over 90 per cent. of the total gold contained in the ore, at a cost of less than 6s. per ton of original ore.

The treatment of slimes, the latest development of the cyanide process, is a sign of the new order of things in the metallurgy of gold. Worked out with the greatest patience and skill by direct experiment on the substances to be treated, and without any thought of exclusive advantage to its devisers, its main features have been at once published for the free use of the rest of the world, and will probably pass into general use with little delay.

T. K. ROSE.

THE NATURE OF ALTERNATION OF GENERATIONS IN ARCHEGONIATE PLANTS.

AN HISTORICAL SKETCH.

THERE are few scientific terms the meaning of which does not become more or less modified with the progress of investigation. Cases of the complete loss of the original significance are, however, less frequent, and when they occur usually involve a period of transition before the use of the term again becomes definitely fixed. The phrase "alternation of generations" as applied to plants has undergone such a change of meaning: this necessitates a brief retrospect of its use in the past fifty years before attempting to consider recent progress in our knowledge and opinions regarding the phenomena now denoted by it.

The term was introduced by Steenstrup,¹ in 1845, to denote "the remarkable and till now inexplicable natural phenomenon of an animal producing an offspring which at no time resembles its parent, but which on the other hand itself brings forth a progeny which returns in its form and nature to the parent animal". Plants are not referred to until the last page of this monograph which deals with various groups of invertebrata, and then the succession of vegetative shoots culminating in the flower or reproductive shoot is recognised as the corresponding phenomenon. The diagrammatic representation of a similar view in the frontispiece of Owen's classic work on *Parthenogenesis* may be referred to in illustration.² That the importance of the succession of sexual and spore-bearing forms in the life history was not recognised can be readily understood, when the fragmentary nature of the available data is remembered. Observations pointing to the discontinuity between these two stages in Bryophyta and Pteridophyta indeed existed, but a comparative treatment was wanting. Even in 1851, Braun³ only uses the term alternation in the sense in which

¹ Steenstrup.

² Owen.

³ Braun (1).

it had been employed by Steenstrup. He, however, discusses at considerable length the life history of mosses and ferns, in which fertilisation is regarded as falling in the middle of the cycle of development, "becoming the means of transition from a lower to a higher stage of the metamorphosis". In a work¹ published two years later similar views with regard to the importance of this alternation of shoots are elaborated in detail; the importance of the sexual or asexual mode of reproduction is, however, recognised, though the cryptogams are intentionally left out of consideration.

But in the *Vergleichende Untersuchungen*² the life histories of the main groups of archegoniate plants were described in detail and treated comparatively; from this time the importance of the regular alternation of a sexual and spore-bearing generation has been beyond question. In this work, and even in the *Higher Cryptogamia*³ published in 1862, Hofmeister made no remarks on the nature of the alternation of generations, the essential unity of which throughout Bryophytes, Pteridophytes and Gymnosperms he clearly demonstrated; neither did he extend the comparison to Thallophytes.

In the interval between these two works of Hofmeister the materials for this comparison had accumulated as the life histories of the Algæ became accurately known. In 1856 Pringsheim had pointed out that *Ædogonium* and *Coleochæte*, in both of which the zygote divides into a number of cells, approached the level of development of the moss, and had compared the many celled fruits of the latter genus with the sporogonium of *Riccia*.⁴ This view is still further developed in later papers⁵ and an important distinction is drawn between the alternation of shoots and true alternation of generations, the succession of individuals by asexual reproduction in *Coleochæte* being regarded as the equivalent of the former. "We see in the *Coleochæteæ*, just as in the pleurocarpic mosses, the succession of shoots or succession of generations along with the form

¹ Braun (2).² Hofmeister (1).³ *Ibid.* (2).⁴ Pringsheim (1).⁵ *Ibid.* (2), (3).¹

of alternation of generations which expresses itself in the fruit-formation."

At this time descent was not a recognised factor in morphological comparison. In reading the clear statements of fact and the definite conclusions arrived at on comparative grounds in the works of Pringsheim and Hofmeister this circumstance is readily forgotten. It applies however to papers published for some years after the first edition of the *Origin of Species*. The importance of descent in such morphological questions was recognised and insisted upon by Haeckel in 1866.¹ He discussed alternation of generations in plants and animals and, though some of his comparisons were misleading, the main conclusions so far as they relate to plants must be mentioned on account of their bearing on later theories. In Bryophyta and Pteridophyta, which he grouped together as Prothallophyta, Haeckel recognised true alternation (Metagenesis), but he compared the protonema with the fern prothallus, and the leafy moss plant together with the sporogonium to the fern-plant. Except for isolated cases of reproduction by separable bulbils he did not consider that true alternation was exhibited by Phanerogams. In the latter the succession of vegetative and reproductive shoots was distinguished from Metagenesis under the name of Strophogenesis or succession of generations.

The influence of Haeckel's views on alternation is clearly traceable in a paper by Strasburger,² which appeared some years later; the conclusions are of interest from a historical point of view, although they are not maintained in later works by this author. He distinguished as true alternation of generations (Metagenesis) the succession in the life history of two or more genealogical individuals which have become more or less unlike. This is recognised as existing in the vegetable kingdom only among Thallophytes. From the Mosses onwards the alternation is considered to be of a different nature (Strophogenesis), and to have arisen by the splitting of the genealogical individuals of a single generation into physiological individuals. Misled by the

¹ Haeckel.

² Strasburger (1).

differentiation of the moss-plant into axis and leaf, and by the view that sexuality must be associated with the more highly developed generation, the attempt is made to derive the fern-plant from the moss-plant alone, the sporogonium having no equivalent in the life history of the fern. This view lost sight of the homologies already recognised by Hofmeister throughout archegoniate plants. While the general statement, that the two generations in these plants arose by individualisation of parts of a single generation, appears to be in agreement with the antithetic theory, which has next to be discussed, the application of the view made in the comparison of Moss and Fern shows that it is essentially different.

The series of papers by Celakovsky and Pringsheim which have now to be mentioned are of special importance, since the problem of the nature of alternation in the archegoniatae assumes in them the form which in the main holds at the present time. An important character, distinguishing these works from nearly all those previously referred to, is that alternation of generations in plants is considered by itself, no attempt being made to fit the phenomena into a classification of the analogous occurrences in animals. In a paper published in 1868,¹ Celakovsky recognises the work of Hofmeister and Pringsheim on the life histories of Algæ and Archegoniatae as the basis for a consideration of the vegetable kingdom as a developmental whole. The sexual generation which is alone represented in most Algæ is termed the Protophyt, while the succeeding generation which produces reproductive cells asexually is the Antiphyt. This "polar alternation of generations" is distinguished from other subordinate forms. Among the Algæ it is found in *Edogonium* and *Coleochæte*. In a subsequent paper² these views are elaborated, and a general survey taken of the phenomena included under the term alternation of generations. In accordance with the views of Braun, but in opposition to those already expressed by Sachs,³ the shoot is recognised as the simplest vegetable individual, and

¹ Celakovsky (1).

² *Ibid.* (2).

³ Sachs.

three forms of alternation of shoots are distinguished: (1) the alternating shoots owe their distinctness to phyllo-morphism; (2) the leafy shoot is produced from a thallome, *e.g.*, moss-plant from protonema; (3) the second shoot generation comes into existence as the result of a sexual act (*Ascomycetes*, *Basidiomycetes*, *Florideæ*.) This third form is termed antithetic alternation of shoots. In contrast to alternation of shoots is placed the alternation of bionts; two essentially different kinds of this are distinguished as antithetic and homologous alternation. In the former, seen in Muscineæ and Vascular Cryptogams, two fundamentally distinct generations are present, the asexual either being immediately itself a fruit body, or as its ultimate aim producing fruits and spores. The existence of independent sexual and asexual plants, so commonly seen in Thallophytes, constitutes homologous alternation. If the alternation of asexual and sexual generations in Algæ be represented by AB, AB the antithetic alternation will be BC, BC. Among Algæ *Coleochæte* and *Oedogonium* possess the three generations A, B, C. The greater definiteness of the succession of the alternating antithetic generations is referred to the fact that the sexual cells and spores are strictly connected with their respective generations. "The protophyt is never able to produce spores as well as sexual cells, the antiphyt never sexual cells besides spores." In homologous alternation on the other hand the same individual may bear sexual and asexual reproductive organs. The difference is explained when it is remembered that the antiphyt was originally a fruit generation, and only later took on vegetative functions in the Vascular Cryptogams. In the concluding part of this paper, the views of Strasburger mentioned above are critically examined.

Alternation of generations was again considered by Braun in 1875.¹ The view taken agrees on the whole with that of Celakovsky, though the author inclines to regard the fruit generation, of *Coleochæte* at least, as homologous with the thallus of the sexual generation. He recognises the importance of antithetic alternation, al-

¹ Braun (3).

though he objects to the terms used by Celakovsky and employs instead embryonal alternation, distinguishing the generations as proembryonal and embryonal respectively. Antithetic alternation is considered to be peculiar to plants, a close analogy to alternation of generations in animals being found in alternation of shoots. The paper is of special interest from the broad philosophical standpoint assumed, which leads to the discussion of several factors in the problem, which have frequently been lost sight of. Thus Braun lays stress on the possibility that the development of a group of organisms may have been monophyletic or polyphyletic and gives point to the general discussion by suggesting that the Mosses are to be considered "as a further and, as it appears, geologically late development from Thallophytes." In discussing the phylogenetic significance of abnormalities, he points out that it is in most cases incorrect to assume that they are atavisms; they are rather of the nature of "morphogenetic possibilities". Lastly, this important paper deserves notice for the picture it presents of the state of "*Unsicherheit und Verwirrung*" in which the question of the nature of alternation had remained during the twenty years after the publication of the *Vergleichende Untersuchungen*.

In the year preceding this paper, the first case of the direct vegetative production of the fern plant from the sexual generation (apogamy) had been recorded,¹ and in 1876, Pringsheim,² after repeated trials, induced the corresponding phenomenon³ of the vegetative production of the sexual from the tissues of the asexual generation in Mosses. These discoveries, the nature of which will be referred to later, led him to a theoretical view of the nature of alternation in archegoniate plants, which is essentially different from the antithetic theory. In the following year,⁴ he developed this theory in detail. Two types of alternation in plants are recognised, (1) the vegetative alternation of shoots exhibited by both sexual and asexual generations, and (2)

¹ Farlow (1).

² Pringsheim (4).

³ This was afterwards termed "Apospory" by Vines (1).

⁴ Pringsheim (5).

the sexual alternation of generations. The latter includes the admittedly homologous alternation in Thallophytes and the antithetic alternation in Mosses and higher plants. Between these, however, Pringsheim recognises a difference of degree only. He regards sporangia, and the sexual organs (antheridia and archegonia in the widest sense) as truly homologous structures, which have proceeded from one another, and considers that this relationship is made manifest "through the representative, correlative succession of generations with spores and of generations with sexual organs. The Thallophyta differ considerably in the relation borne by the sexual and asexual individuals to one another. In some, no alternation is found. The most common case is that a sexual generation alternates with a succession of neutral generations, the last of which again produces the sexual form. The dimorphic character is, as a rule, exhibited only in the reproductive organs, the generations resembling one another in vegetative structure, and in the possession of subordinate forms of multiplication. In a number of cases, however, the first neutral generation differs more or less widely from those succeeding it. In several genera this generation is reduced to a sporangium, the spores from which may resemble in general appearance those derived from the neutral generation with developed vegetative parts. *Oedogonium Coleochæte* and *Cystopus* present in their fruit body, according to Pringsheim, such a reduced first neutral generation. In yet other algæ (*e.g.*, *Sphæroplea*), this is the only neutral generation found, and the alternation is a definite one between a single sexual and a single spore-bearing generation. The organisms, just mentioned, are compared by Pringsheim, as well as Celskovsky, with the simplest Bryophyte sporogonia. But the view, indicated above, of the phylogenetic history of the fruit body, of *Coleochæte* for example, leads to a conclusion as to the nature of alternation in the archegoniates essentially different from the antithetic theory. The phenomena of Apospory and Apogamy are considered by Pringsheim to support this conclusion that in the Bryophytes and Pteridophytes, we have simply a special case of homologous

alternation. The question of the mode of origin of the complicated and ultimately independent sporophyte of the latter group is not entered upon by Pringsheim.

The antithetic theory was restated¹ in the light of Pringsheim's views in 1877. Celakovsky recognises clearly the two main points contended for by that author, but while he admits that the fruit of *Florideæ*, *Ascomycetes* and *Hymenomycetes* does not represent the second generation (antiphyt), he maintains the view that the antithetic alternation in Mosses and Vascular Cryptogams is essentially distinct from the homologous alternation of Thallophytes. Certain additions to the antithetic theory must be mentioned, since they are the basis on which later work has in great part proceeded. The fruit body of *Coleochæte* is not recognised as a generation since all its cells produce swarm spores, but it is pointed out that from the *Coleochæte* fruit to the *Riccia* sporogonium is but a step. "This step is made in this way, an outermost layer of the spore-producing parenchyma transforms itself into a covering layer (wall of the sporogonium) and thus remains sterile instead of its cells changing to spores." The extension of this modification of spore-producing cells would lead to the kind of sporogonium found in the higher Bryophyta. Thus the Moss fruit is not homologous with the neutral generation of a Thallophyte, but is "a third newly arrived generation interpolated between the sexual and the first neutral generation". To illustrate this the series of forms *Vaucheria*, *Ædogonium*, and *Cystopus*, *Coleochæte*, and *Riccia* are used. Celakovsky does not admit the importance which Pringsheim attached to the phenomena of Apospory and Apogamy, as evidence of the homology of the sexual and spore-bearing generations. The former is to be explained by the origin of all the vegetative cells of the sporophyte from primitively reproductive cells, while Apogamy proves nothing further than that the second generation can arise from an indifferent cell of the prothallus, instead of from a special sexual cell by a sexual process. He points out that in these cases the alternation is not lost, the archegonial cell

¹ Celakovsky (3).

(ovum) does not give rise to a prothallus or the sporogonium stalk to a new sporogonium. The threefold alternation, which as the title of this paper indicates is recognised in the vegetable kingdom, may be mentioned in conclusion. The forms are: (1) Homologous alternation in the Thallophyta between two or more Protophyt generations. (2) Antithetic alternation between Protophyt and Antiphyt in the Mosses and Vascular Plants. (3) Homologous alternation between two or more Antiphyt generations, *i.e.* alternation of shoots in vascular plants, and especially in phanerogams.

Summing up the state of the question at this date (1878) we find that, owing in great part to the work of Pringsheim and Celakovsky, the morphological problem presented by the alternation of generations in archegoniate plants had been clearly recognised. It was seen that the true nature of alternation in Mosses and Ferns was only to be ascertained by arriving at correct views of the manner in which these groups had descended from lower forms such as those represented by the Thallophyta. On the one hand this might have taken place by the further development of a generation equivalent to the sexual generation, the individuals of which had become more or less reduced and remained in connection with the parent plants. On the other hand the elaboration of the product of fertilisation might have been an entirely new development, the result of which did not represent what had at any period been an independent generation. The further development of this interpolated generation with the appearance of vegetative organs might have proceeded by some of the spore-producing cells becoming sterile. On the first theory the parts of the second generation as well as the generation itself might exhibit homologies with the sexual generation on the latter homology (in the sense of homology by descent) is out of the question. It was as a subordinate extension of the homologous theory that Pringsheim¹ considered actual homology to exist between the seta of a moss capsule and the stem of the leafy plant.

Deferring until afterwards the consideration of theoretical

¹ Pringsheim (5).

views as to the nature of alternation which have been expressed since 1878, the results of a number of investigations, which have a more or less direct bearing on the question, may be first considered.

INVESTIGATIONS INTO THE NORMAL LIFE HISTORIES OF THALLOPHYTES AND ARCHEGONIATES.

The life histories of the main groups of Bryophyta and Pteridophyta, and of the Thallophytes which present the closest resemblance to them in this respect, were already so fully known that comparatively few facts of this kind bearing directly on the nature of alternation have been recorded since 1878. The important work of Klebs,¹ however, an account of which has already been given in SCIENCE PROGRESS,² has gone far to place the question of the nature of alternation in Thallophyta on an experimental basis. Klebs has shown that the alternation of the free living generations in a number of Algæ can be controlled by suitable modifications in the conditions of cultivation, and that, in *Ædogonium*, *Hydrodictyon*, and *Vaucheria* for example, the development of sexual or asexual organs of reproduction can be determined by the investigator. This places beyond any doubt the homology of the sexual and the free neutral generations in these Algæ, but it leaves untouched the all-important question whether or not the first neutral generation (to use Pringsheim's terminology) is or is not homologous with the other generations in the life cycle. To establish this homology, it will be necessary to show that the zygote of *Ædogonium* or *Coleochæte* can be induced to develop directly into the corresponding thallus, bearing sexual or asexual reproductive organs. The work of Klebs as yet published, although it does not settle this question, is of the highest importance, since it suggests the possibility of direct experimental evidence being obtained upon it in the Algæ at least. In this connection the work of Goebel,³ Dodel Port,⁴ and Klebs⁵

¹ Klebs (2).

² Ward.

³ Goebel (3), (4).

⁴ Dodel Port.

⁵ Klebs (1).

may be mentioned, since in it we find the first steps made in the experimental study of the gametophyte of mosses and ferns.

Treub's investigations¹ into the structure of the prothallus, and the development of the young plant in the genus *Lycopodium* have brought the close resemblance that may exist between the sporophyte and the prothallus of a Vascular Cryptogam into prominence. It is, however, a difficult question to determine how far morphological importance can be attached to the fact.

The work of Bower² on the normal development of Vascular Cryptogams, and especially of their spore-producing members has given considerable support to the antithetic theory, by showing that the structural facts in connection with the more recent modifications of these plants would bear the interpretation which that theory assumes. Omitting for the present the consideration of the theoretical views to which these investigations have led, the chief fact bearing on the question we are considering is that sterilisation of potentially spore-producing tissue has been shown to occur in the sporogonia of Bryophyta and in the sporangia of Vascular Cryptogams and Angiosperms. The complicated spore-producing structures seen in some of the latter groups may be viewed as having been derived from simpler sporangia in essentially the same way as the antithetic theory assumes the first stages of development of the sporophyte to have taken place. This view, originally suggested by Celakovsky, has been elaborated by Bower, and makes it possible to understand how the passage may have occurred from the wholly dependent Bryophyte sporogonium to the plant in the Vascular Cryptogams which is only at first dependent on the gametophyte. This comparison has been facilitated by fuller knowledge of the structure of the simpler sporophytes of the latter group especially that of *Phylloglossum*,³ the full life history of which is unfortunately not yet known.

¹ Treub (1).

² Bower (13).

³ *Ibid.* (2).

DEVIATIONS FROM THE NORMAL MODE OF ALTERNATION OF GENERATIONS.

The fact has already been mentioned that it was the discovery of apospory in mosses, together with the earlier discovery of apogamy in ferns, which led to the theory of homologous alternation being stated. Further investigation has strengthened the evidence to be derived from these deviations from the normal life history if—and this is still a disputed point—they afford any valid evidence at all.¹

Little further has become known of apospory in mosses since Pringsheim² induced it in *Hypnum cupressiforme*, *H. Serpens* and *Bryum caespitosum* and Stahl³ confirmed his observations in *Ceratodon purpureus*. These experiments do not appear to have been further extended, but the interesting fact has been recorded⁴ that a similar phenomenon may occur in sporogonia of *Funaria hygrometrica* while still attached to the moss plant, which was growing in a natural state. The sporogonia in question were found with atrophied capsules buried in the soil and giving rise to protonemal filaments. Such a case approaches closely to the hypothetical future development of a moss plant imagined by Pringsheim in 1877. No further investigations have been made to determine whether the gemma-like bodies found in capsules of *Eucamptodon perichæticalis*⁵ were aposporously developed as seems not improbable from the brief description.

The corresponding phenomenon in ferns, anticipated on theoretical grounds by Pringsheim,⁶ has been made known and investigated in a number of species. Discovered by Druery⁷ in *Athyrium Filix-fœmina* and by Wollaston⁸ in *Polystichum angulare*, these early cases were fully investigated by Bower.⁹ Apospory is now known in nine species, viz:—

¹ On the general question of the value of abnormalities in morphology, see Goebel (6), pp. 152 *et seq.*

² Pringsheim (4), (5).

³ Stahl.

⁴ Brizi.

⁵ Montague.

⁶ Pringsheim (5).

⁷ Druery (1).

⁸ Wollaston.

⁹ Bower (1), (3).

Onoclea sensibilis.¹*Athyrium Filix-fœmina*.²*Aspidium (Polystichum) angulare*.³*Nephrodium Filix-mas*.⁴*Scolopendrium vulgare*.⁵*Pteris aquilina*.⁶*Trichomanes alatum*.⁷*T. pyxidiferum*.⁸*T. Kaulfussii*.⁹

It is unnecessary to give details of the phenomenon in the different species, but the general facts may be summarised. The prothalloid growths have been found to arise from the leaves or from young sporangia; in a number of cases their production is associated with a more or less complete sporal arrest. When the growth does not proceed from the sporangium it may occur from cells of the placenta, from the leaf-margin or from superficial cells of the leaf above the termination of a vascular bundle. Sometimes this commences while the leaves are still attached to the plant and standing erect; in other cases it has been induced by laying portions of the frond on damp soil. Special interest attaches to cases of apospory described in *Nephrodium Filix-mas*¹⁰ and *Scolopendrium vulgare*¹¹ in which the prothalli arose from or near the margin of the early formed fronds of the sporophyte. Since fronds of this age do not in these species bear sporangia, sporal arrest can hardly have been a factor in the causation of the apospory. In *N. Filix-mas* the young plant was apogamously produced and the prothalli on its fronds were also apogamous.

Attempts to induce apospory by laying portions of fronds on damp moss have been made without success,¹² but indications of the effect of interference with normal conditions of life in at least predisposing to apospory are not wanting. Thus the prothalli of *Scolopendrium*, a number of which produced aposporous plants, had been repeatedly subdivided.¹³ The *Onoclea* the aposporous growths formed upon sporophylls which had been induced to assume the characters of the sterile fronds by the removal of the latter from the plant. This may be regarded as an instance of experimental sporal arrest.¹⁴

¹ Atkinson (1), (2).² Druery (1), (4).³ Wollaston; Bower (1).⁴ Druery (3), (4).⁵ *Ibid.* (4); Lowe.⁶ Farlow (2).⁷ Bower (5).⁸ *Ibid.* (12).⁹ Lowe.¹⁰ Druery (3), (4).¹¹ Lowe.¹² Bower (6).¹³ Lowe.¹⁴ Atkinson (2).

Apogamy which was discovered by Farlow in *Pteris cretica*¹ in 1874 and investigated in detail in this and two other species by De Bary² in 1878, is now known in a considerable number of species of Ferns. As in the case of apospory a list of these will be given and the general nature of the phenomena briefly considered.

Todea africana.³

T. rivularis.⁴

T. pellucida.⁴

Osmunda regalis.⁵

—————
—————

Ceratopteris thalictroides.⁵

Pteris cretica.⁶

(?) *Pt. serrulata*.⁷

Nephrodium Filix-mas.⁸

N. falcatum.⁹

N. dilatatum.¹⁰

N. Oreopteris.¹⁰

Aspidium aculeatum.¹⁰

A. angulare.¹⁰

A. frondosum.¹⁰

Doodya candata.¹¹

D. Aspera.¹⁶

Athyrium Filix-fœmina.¹²

A. Niponicum.¹⁰

Scolopendrium vulgare.¹⁰

Notochlæna distans.¹³

—————

Trichomanes alatum.¹⁴

—————

(?) *Botrychium virginianum*.¹⁵

Apogamy may be defined, in the light of our knowledge of the cases in the above list, as the direct vegetative production from the prothallus of a complete sporophyte, or of any member or group of members of the latter, or of tissues characteristic of the sporophyte within the prothallus. In the first described cases a perfect sporophyte, which continued its growth in the normal manner, was formed on the under surface of a flat prothallus of the ordinary shape. There was more or less complete absence of archegonia from the prothallus in the substance of which tracheides were developed. A comparative review of the known cases shows however that they form a series as regards the directness of the origin of the sporophyte. It is convenient to describe the phenomena in this order, although it must be remembered that a single culture may show several of the forms here distinguished. Changes in the form of the

¹ Farlow (1).

² De Bary.

³ Sadebeck.

⁴ Stange.

⁵ Leitgeb.

⁶ Farlow (1); De Bary.

⁷ Trow.

⁸ De Bary; Kny. Lang (2).

⁹ De Bary.

¹⁰ Lang.

¹¹ Stange; Heim.

¹² Druery (5); Lang (2).

¹³ Berggren.

¹⁴ Bower (5).

¹⁵ Jeffrey.

¹⁶ Mentioned by Sadebeck (2) p. 34.

prothallus usually occur first; it may become thick and fleshy and a cylindrical process may grow from the anterior margin or the under surface. Tracheides develop in the tissues of the prothallus before any other manifestation of apogamy. The apex of the cylindrical process may continue directly as the apex of a fern plant. In other cases isolated members of the sporophyte (ramenta, roots, leaves, sporangia¹) may be developed from the process, which usually contains an axial strand of tracheides. In other cases (and sometimes in association with a cylindrical process) shorter conical projections develop from or around the sexual organs. One or many of these may give rise to sporophytes. All these peculiarities have been observed in prothalli, which would have produced normal embryos had fertilisation been permitted. This induced apogamy contrasts with the cases of direct apogamy in which the prothallus as soon as it has attained a certain size, and apparently independently of the conditions, is able to produce a sporophyte apogamously. Intermediate examples however occur which indicate that direct apogamy is only a special case of the capability which many normal prothalli show of producing the fern plant asexually. Apogamy may in seems be induced in many, possibly in nearly all, ferns by preventing fertilisation in prothalli growing under conditions favourable to nutrition. The cause of direct apogamy is still obscure.

With regard to early references to apogamy, the work of Wigand² must be mentioned. A careful study of his paper and figures has led the author to the conclusion that it is probable some of the prothalli he used in the course of his investigations were apogamous. In this fact the explanation of his opposition to Suminski's description of the functions of antheridia and archegonia may be found. The quotation from Wigand's paper given by Farlow³ has, however, no reference to apogamy, but clearly relates to gametophytic budding. Mercklin's observation

¹ Sporangia developed from the prothallus are as yet known only in *Scotopendrium vulgare* and *Nephrodium dilatatum*.

² Wigand.

³ Farlow (1).

of tracheides in the prothallus may also be mentioned here.¹

The relation which exists in some cases between apospory and apogamy on the one hand, and the much more common phenomena of sporophytic and gametophytic budding on the other deserves to be referred to in passing; further facts are required before any definite conclusions can be drawn. Certain examples of sporophytic budding have been regarded as extreme cases of apogamy resulting in the entire omission of the sexual generation from the life cycle.² In no case, however, does the evidence in favour of this interpretation seem to be sufficient. No really satisfactory cases of apogamy or apospory occurring in ferns in the natural state have yet been recorded. Apospory in *Pteris aquilina*,³ on the prothalloid growths of which sexual organs were not observed, and the case of the occurrence of tracheides in a prothallus of *Botrychium virginianum*⁴ were observed in wild plants. A number of aposporous and apogamous ferns are descended from wild finds, but in them it is uncertain whether the peculiarity has not been acquired under cultivation.

Treub⁵ has recently recorded a case of apogamy in an Angiosperm (*Balanophora elongata* Bl.), the embryo of which develops without fertilisation from cells of the endosperm. The details of this interesting observation will be referred to below.

NUCLEAR DIFFERENCES BETWEEN THE ALTERNATING GENERATIONS.⁶

The recognition of a difference between the nuclei of the cells of the sexual and asexual generations constitutes

¹ Mercklin, p. 54.

² Goebel (1).

³ Farlow (2).

⁴ Jeffrey.

⁵ Treub (2).

⁶ No attempt has been made to give a complete list of the literature relating to the reduction of chromosomes in plants, and only those papers which are necessary to illustrate the bearing of the general result on alternation are mentioned. Full references will be found in Strasburger (2) and (3) and in Zimmerman, *Morphologie u. Physiologie d. pflanzlichen Zellkernes*.

one of the most important additions to our knowledge of the facts of alternation. This is manifested by the number of chromosomes which are distinguishable in the dividing nucleus being twice as great in the cells of the sporophyte as in those of the gametophyte. This has now been established for representatives of the Bryophyta, Pteridophyta, Gymnosperms and Angiosperms, and, although the results need to be extended, may reasonably be assumed to hold throughout these groups. The increase in number of chromosomes takes place as the result of the sexual fusion; the double number is retained in the vegetative cells of the sporophyte; the reduction occurs in the spore mother cells. The existence of the double number of chromosomes in the cells of the sporophyte affords support to the antithetic theory of alternation, in that the spore-bearing generation appears as an interpolation, not merely between two successive gametophytes, but between the sexual fusion and the reduction in number of the chromosomes. The bearing of the facts known as to this periodic reduction of chromosomes upon the nature of alternation has been considered by Strasburger¹ in two important papers. His general conclusion may be given: "The morphological cause of the reduction in number of the chromosomes and of their equality in number in the sexual cells, is in my opinion phylogenetic. I look upon these facts as indicating a return to the original generation from which, after it had attained sexual differentiation, offspring was developed having a double number of chromosomes."

It thus becomes of great importance to ascertain whether corresponding phenomena take place in those Thallophyta which approach the Bryophyta most nearly in their alternation of generations. The facts are still unknown for such Algæ as *Oedogonium* and *Coleochæte* and in the only Alga yet accurately investigated (*Fucus*)² the reduction takes place just before the development of the sexual cells. *Fucus*, however, cannot be regarded as bearing directly on

¹ Strasburger (2), (3). See also the earlier paper by Overton.

² Farmer and Williams. See also Strasburger, *Jahrb. Wiss. Bot.*, 1897, p. 351.

the question at issue. In *Cystopus*, however, the life history of which was regarded by Celakovsky as corresponding to antithetic alternation, the nuclei in the developing oospore appear to possess twice the number of chromosomes present in those of the vegetative thallus. Reduction apparently takes place just before the division of the thirty-two nuclei which proceed from the nucleus of the zygote. From these about four times as many zoospores are produced. The details of this last division are not known but the analogy which it presents to tetrad division is obvious.¹

The recent discovery of a sexual nuclear fusion preceding the formation of the fruit body of Ascomycetes has raised anew the question whether these plants do not exhibit an antithetic alternation. This has been discussed by Harper² but a definite conclusion is prevented by the absence of the necessary observations on the number of chromosomes present in the nuclei of the hyphæ before fertilisation.

While these facts regarding normal alternation appear to be most readily explained on the antithetic theory, it must not be forgotten that the phenomena of apogamy and apospory show that the nuclear changes (which presumably occur in these developments also) are not necessarily associated with the sexual fusion or spore formation. Nothing is known of the behaviour of the nuclei in these cases and it is unnecessary to discuss the question on *a priori* grounds in this place since this has been fully done by Strasburger³ and Bower.⁴ From this standpoint the case of apogamy in *Balanophora elongata*⁵ possesses especial interest. The facts are briefly these. As the result of division of the primary nucleus, four nuclei are present at each end of the embryo sac. Three of these at one end belong to the ovum and the two synergidæ. The entire egg-apparatus and the four nuclei at the antipodal end of the embryo sac abort, taking no further part in the development. This proceeds from the polar nucleus at the egg-

¹ Wager, Berlese.

² Harper.

³ Strasburger (2).

⁴ Bower (11).

⁵ Treub (2).

apparatus end, which undergoes division *without previous fusion with the corresponding nucleus*. Within the prothallus which thus results the embryo is developed apogamously from a cell which occupies a certain position. It is of considerable importance as bearing on the nuclear changes which precede apogamy that the fusion of the polar nuclei (which in other cases is known to result in the doubling of the number of chromosomes in the nuclei of the endosperm) does not take place. The exact determination of the nuclear changes in this or similar cases would be of great interest.

THE DEVELOPMENT OF THE THEORIES OF ANTITHETIC AND HOMOLOGOUS ALTERNATION.

The nature of the alternation in Thallophytes was discussed by Vines¹ in 1879. His conclusion was that it is impossible to detect any distinct alternation in any but the *Coleochæteæ* and *Characeæ*. In the latter the pro-embryo was regarded as the sporophyte, the main shoot representing an aposporously produced oophyte.²

The views of Naegeli³ differed from those of both Celakovsky and Pringsheim as to the divisions of the life cycle of the Thallophyta which are to be regarded as generations, and in the comparisons instituted between these and the two generations of Archegoniate plants. A full discussion is impossible, but the views expressed with regard to the alternation in *Ulothrix* or *Ædogonium* on the one hand and the Moss or Fern on the other will render the main point clear. The *Ulothrix* or *Ædogonium* plants reproducing asexually by zoospores are the repetition generations ($B_1 - B_n$); they are followed by one composed of individuals in the main similar to them, but bearing the sexual organs

¹ Vines (2).

² Vines (1), (2), (3). In connection with this view the fact that a reduction of chromosomes does not precede the development of the spermatozoids may be mentioned (Debski). Compare also Strasburger, (3), p. 413.

³ Naegeli.

(the sex-producing generation C). The male and female reproductive cells and the zygote resulting from their union constitute the androgynous generation (D). This after a period of rest produces swarm spores, each of which develops into and represents the first stage of an individual of the sexually produced generation (A). The alternation is thus expressed

$$A. B_1 - B_n. C. D.$$

The sexual generation of the moss (protonema and moss plant) is regarded as corresponding to the repetition generations and the sex-producing generation in *Ulothrix*; the sporophyte to the androgynous generation and the sexually produced generation. Using the same symbols as before, the life history of the moss is represented by

$$(B_1 - B_n + C) \quad (D + A).$$

While both in the antithetic and homologous theories the correspondence between the group of spores formed from the zygote in *Ulothrix* or *Ædogonium* and the moss sporogonium is recognised, on Naegeli's view the result of division of the zygote in these Algæ is not regarded as a generation. This introduces confusion into the comparison of the life history of the green Algæ with that of archegoniate plants. The application of these views to the problem of the probable course of evolution followed in the origin of Bryophyta and Pteridophyta from Algal ancestors appears to accord rather with the antithetic theory, though, as the comparison of the life histories given above show, points in common with the homologous theory also exist.

Some of the facts bearing on the nature of alternation which have been investigated by Bower have already been referred to; it remains, however, to indicate the main lines on which his elaboration of the antithetic theory has proceeded, and the views as to the phylogenetic relations existing between Algæ, Bryophytes and Pteridophytes expressed by him.¹ Agreeing with the distinction drawn by Celskovsky between antithetic and homologous alternation, and regarding the former as having originated by the inter-

¹ Bower (4), (7), (8), (9), (10), (13).

polation of a stage in the life history, Bower has traced the probable course of evolution of the sporophyte as illustrated by such a series as *Edogonium*, *Coleochæte*, *Riccia*. But, going farther than Celakovsky, he has shown strong reasons for considering the increase in the complexity of the Bryophyte sporogonium to have been due to progressive sterilisation, and has extended this comparison to the lower Pteridophyta. The point of contact with ancestral forms, the sporophyte of which was of similar construction to a Bryophyte sporogonium, is sought among strobiloid types,¹ such as Lycopodineæ and Equisetineæ, and the working hypothesis is put forward "that in the strobiloid Pteridophyta the apex of the sporogonium is the correlative of the apex of the strobilus". In the transition from the sporogonial to the strobiloid form progressive sterilisation with differentiation of the sterile tissue, the formation of appendicular organs, and the subdivisions of the archesporial layer to form isolated patches instead of one continuous tissue are assumed to have taken place. In addition Bower has indicated the relation borne by these changes to the alteration in mode of life, which is assumed to have taken place on the spread of plants to the land, and has thus brought the biological aspect of the subject into proper prominence.

The relation of the course of evolution to the probable conditions is also dealt with by Atkinson,² who makes the additional suggestion that the disturbance of the assimilatory function of the gametophyte, induced by its spread to the land, would not only assist the sterilisation of some of the sporogenous tissue, but would tend to force the function of assimilation upon some of the sterilised regions of the sporophyte.

The views of Strasburger have been referred to above, and it is sufficient to say that on the ground of the facts known as to the periodic reduction of chromosomes he arrives at conclusions which are in essential agreement

¹ Goebel, on the other hand, has compared Mosses directly with the Ferns on the basis of resemblances in the sexual generation. Goebel (5).

² Atkinson (2), (3).

with those previously stated by Celakovsky and Bower as the result of morphological comparison.¹

The nature of alternation has also been considered by Macmillan² and Fry,³ but a consideration of these and other views would not aid in the elucidation of the main problem, since they differ mainly on points of detail.

Scott⁴ has given a most suggestive review of the two alternative theories of alternation in his presidential address to the Botanical Section of the British Association. The critical nature of this work necessitates a reference to the original, but the important bearing of his clear restatement of the homologous theory upon the present state of opinion on the nature of alternation must be pointed out.

CONCLUSION.

To attempt a general discussion of the nature of alternation of generations and its relation to the successions of forms which have been distinguished as generations among the Thallophyta is not within the scope of this article. Some of the factors in the problem will, however, be referred to, and some lines of investigation, the results of which may be expected to aid in its solution, indicated in conclusion.

A study of the literature has made it evident that the facts have been interpreted very differently by the investigators who have discovered and discussed them. If we attempt to determine what is the essential distinction between the theories of homologous and antithetic alternation, the conclusion arrived at will, it appears to me, be of this nature. On the homologous theory, the sporophyte is to be traced back to a generation of originally independent individuals similar to those from which the gametophyte has arisen, the almost invariable alternation and the permanent or temporary dependence of the spore bearing on the sexual generation being subsequent adaptations. On the antithetic theory, the sporophyte is not derived from free-living in-

¹ Strasburger (2), (3).

³ Fry.

² Macmillan.

⁴ Scott.

dividuals of the ancestral algal form, but has had a distinct phylogenetic history as an interpolated stage in the life history. On the former view, the two generations of a moss are equivalent to two independent individuals of, *e.g.*, *Ædogonium*, on the latter to one individual and the zygote which produces the spores. If the first neutral generation of the ancestral form had, as Pringsheim's comparisons would suggest, become reduced to a group of spore-producing cells, the methods of advance in the complexity of the sporophyte need not have differed from those assumed by the advocates of the antithetic theory. But it is also possible that the differentiation of the two generations proceeded at first in free living individuals which only later became united to one another in almost invariable sequence. A provisional hypothesis as to how this might have occurred in such a group as the Ferns has been suggested by the author.

The views held as to the probability that one or the other course has been followed are intimately related to those on the connections by descent that exist between the different phyla of the vegetable kingdom. The usual view is that forms like the simpler liverworts descended from green algal forms which are represented by such existing Algæ as *Ulothrix*, *Ædogonium* and *Coleochæte*. From the simple sporophyte of these forms those of the more complex liverworts and the mosses on the one hand and of the ancestors of the vascular cryptogams on the other were evolved.¹ Even such a view involves the independent origin in the different groups of common characters of the sporophyte. But it is not inconsistent with any known facts to go a step farther, and to consider the possibility of a number of somewhat similar developments having taken place from the algal ancestry leading to various forms of simple sporophyte, some of which were physiologically independent after a time, while others were wholly dependent on the gametophyte. In particular, the possibility of the sporogonium of Bryophyta and the sporophyte in the

¹ Goebel while tracing the Pteridophyta to forms resembling the liverworts states clearly his opinion that their asexual generation proceeded on a different line from the commencement. Goebel (2), p. 401.

Vascular Cryptogams having arisen independently of one another must be considered. Such a view, while it does not prevent the use of the stages of complexity of the Bryophyte Sporogonium as illustrations of the probable steps by which the Pteridophyte Sporophyte was evolved, suggests the alternative course of looking for evidence which may indicate how the latter could have been derived directly from an algal form.

The modifications which the recognition of this possibility would entail in current views on homology cannot be touched upon here farther than to point out that the comparison of those forms of sporophyte which there are good grounds for concluding are homogenetic¹ with one another must be relied upon in preference to comparisons between forms which may be merely homoplastic.¹ General impressions, gathered from a wide survey of the relation of sexual and asexual generations in the vegetable kingdom, must be checked by comparisons limited as far as possible to closely related forms.

These considerations suggest further investigation of the behaviour of the sexual generation of the vascular plants in the hope that a more accurate knowledge of the changes which ensue on exposure to altered conditions of life may aid in arriving at conclusions as to how the sporophyte might have been evolved from organisms which in form, and possibly in physiological properties, resembled the gametophyte of existing plants. The changes of conditions to which most importance should be attached are such as may reasonably be supposed to have occurred during the evolution of land plants. From this point of view, considerable importance may be attached to apogamy, and in a less degree to apospory.

The existence of a nuclear distinction between the two generations is not necessarily inconsistent with such an origin of the sporophyte from forms homologous with the sexual generation. For it is an assumption that the nuclear

¹ Lankester. The extension of the use of these terms and of the distinction which they imply would do much toward clearing our ideas on many morphological questions.

difference which has been established is causally related to the origin of the second generation. It is quite possible that it may have been simply coincident with the germination of the zygote *in situ* without a previous rejuvenescence or division into swarm spores.

All progress in our knowledge of the relationships of the various groups of plants may aid in arriving at some determination as to the early course of evolution of the sporophyte. But in addition to the comparative morphology of existing forms, the experimental study of the Green Algæ, the Liverworts, and the simpler Vascular Cryptogams may be expected to yield important evidence on this question. The attempt to induce the zygote in *Ædogonium* or *Coleochæte* to develop directly into a sexual plant, and the experimental study of apogamy and apospory may be mentioned as bearing on the truth of the homologous theory. On the other hand, the experimental causation of sterilisation of spore-producing tissue of simple sporophytes by hindering the nutrition of the latter, as suggested by Atkinson,¹ might yield results confirmatory of the antithetic theory. Such induced changes, though merely "morphogenetic possibilities," would afford, if used with care, satisfactory guides to speculation. The fact that the changes are in many instances sudden and discontinuous need not in the light of recent work on variation exclude them from this use.

In concluding this outline of the history of the theory of alternation of generations in plants, hope may be expressed for a partial solution in the future, though this may be far distant. The clear recognition of the openness of the question is the best safeguard for the facts which support one or other theory having their proper weight accorded to them. The spirit in which such investigation should be carried on has been well expressed by Dr. Scott: "Let us in the presence of the greatest mystery in the morphology of plants keep an open mind, and not tie ourselves down to assumptions, though we may use them as working hypotheses".

¹ Atkinson (2), p. 180.

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THE FALL OF METEORITES IN ANCIENT AND MODERN TIMES.¹

IN matters of scientific evidence relating to events which took place in early times nothing is more difficult than to place oneself in the position of a contemporary critic, amid the mental atmosphere of the time, and to regard the occurrence as it then appeared. One cannot help criticising it in the light of subsequent events, and early observers are, in consequence, too often condemned as credulous. In justice to our predecessors and to clear our own vision it is often profitable to review the development of some article of scientific belief, and to trace the steps by which it has been established.

In the case of meteorites and the belief in their fall from the sky, the story is a curious one, for this belief, though well founded and ultimately justified, for centuries met with opposition or disregard, not from ignorant people, but from the leaders of scientific thought.

The fairest, and doubtless the most interesting, way to gain a picture of the evidence available 100 years ago, of the impression which it produced upon thoughtful men, and of the reasoning by which they were ultimately converted, is to quote verbatim the vivid accounts of eye-witnesses, and the comments which they excited at the time.

The following fragmentary notes contain nothing new, except that some dispersed references are perhaps for the first time brought together.

By way of preface we may collect the main features of the evidence historical and contemporary as it presented itself to our ancestors towards the close of the last century.

Ancient literature, of course, abounds with references, some certain and some dubious, to the fall of stones from the sky ; the great stones that fell from heaven in the battle of Gibeon, the hailstones and coals of fire of the eighteenth Psalm, are among the earliest ; a Chinese account relating to the year 211 B.C. describes the fall of a star which

¹ A lecture delivered in Magdalen College, Oxford ; 19th Feb., 1898.

turned to stone as it fell ; and still earlier Chinese records go back to the date B.C. 644. In the Talmud is a legend concerning the plague of hail in Egypt, that the hailstones were very large, each of them being about the size of an infant's head ; and that as they touched the ground they burst into flames. Livy mentions several instances of a rain of stones, and in the earliest reference which he makes, in his first book, to the shower of stones that fell about 652 B.C. on the Alban Mount he is careful to distinguish them from hailstones, "*haud aliter quam quum grandinem venti glomeratam in terras agunt, crebri cecidere cœlo lapides*".

The best established and the most famous of all in ancient times is that which fell about the time of the battle of Ægos Potami in B.C. 403, and near the scene of the battle, as related by Plutarch in his life of Lysander.

Plutarch says that it was of great size and was held in great veneration by the people of the Chersonese who showed it in his own time. This fall is rendered doubly interesting by its association with the name of the philosopher Anaxagoras who is said to have foretold the event. On this subject Bayle in his Dictionary quotes Philostratus as attributing to Anaxagoras a great reputation for such predictions. At one time he predicted that on a certain day at noon the sun would become dark ; at another he went to the Olympic Games with a cloak, knowing that it would rain, although the day was quite clear and serene ; and a little while after it rained violently.

As is well known, the fall at Ægos Potami is still further confirmed by Pliny, who asserts that the prediction of Anaxagoras was made sixty-two years before the battle. He goes on to say : "The stone is still shown, of the size of a crowbar, and of a burnt colour. There was a comet at night at that time ;" and further : "A stone is at the present day held in reverence at the school of Abydos ; it is only small in size, but it is the one whose fall to the earth was foretold by Anaxagoras. It is also revered at Cassandria, now called Potidœa."

There can hardly be any doubt that, in spite of the legend about its prediction, all this refers to a real meteorite.

The criticism of Plutarch himself on the subject is interesting. He suggests that “ shooting stars are really heavenly bodies which from some relaxation of the rapidity of their motion or by some irregular concussion are loosened, and fall not so much upon the habitable part of the earth as into the ocean, which is the reason that their substance is so seldom seen”.

Aristotle in his chapter on meteors has some remarks on this event in which he seems to regard the stone as having been blown by the wind ; but Plutarch, who discusses the theory held by some in his own time, according to which the stone was really torn by a hurricane from the top of a mountain, expressly rejects this theory.

Among these early accounts we find several accurate descriptions of all the phenomena which are now known to accompany the fall of a meteorite ; the bright light, the noise of thunder or an explosion ; and the stone itself is correctly described as of two kinds, either as a stony substance with a burnt black surface, or as metallic iron.

Thus in the chapter preceding that in which he describes the *Ægos Potami stone*, Pliny mentions the fall of a piece of *iron* among the Lucani in the year before Crassus was killed by the Parthians, and he describes this as being “ *spongiarum fere similis* ” ; this expression at once recalls the aspect of several meteoric irons, notably that known as the Pallas iron which we shall have occasion to mention again.

It is indeed more than probable that most of the iron used by primitive people who have not learnt the art of treating iron ores was derived from such masses of meteoric iron ; and it is to be noticed that in Siberia, Mexico, Chili and Arabia lumps of such material were not only used for weapons, but were much prized on account of their reputed heavenly origin ; Barrow in his voyages reports a mass of this sort found in the mountains behind the Cape of Good Hope which was used in this way.

In this connection an interesting correspondence took place in 1870 between Sir John Herschel and the eminent Viennese mineralogist von Haidinger, relating to the epithet *αὐτοχόωνον* or “ self-fused ” applied to the iron quoit in the twenty-third book of the Iliad ; the word is translated

“rudely cast” by Liddell and Scott, but it has been suggested that it means “native” as opposed to forged iron. Still more curious are two lines mentioned by Eustathius as interpolated near the opening of the fifteenth book of the Iliad relating to two *μύδροι* or “lumps” cast by Zeus upon Troy, ὄφρα πέλοιτο καὶ ἔσσομένοισι πυθέσθαι; and Eustathius adds: “Lumps of this kind are pointed out by the Periegetæ who call them anvils fallen from heaven”.

In addition to the more or less direct evidence of which the preceding are examples there is abundance of indirect evidence derived from the worship of stones; for this worship must, I think, have at least sometimes originated in a meteoric fall.

Jevons in his *Introduction to the Study of Religion* traces the origin of stone-worship and of the anointing of stones merely to the veneration of those which had been used as altars, and this appears to be the opinion of most authors upon the subject. But, although it is by no means probable that most or even many of the holy stones were meteorites, it is more than probable that when so remarkable an event as the fall of a stone from the sky did take place it must have provoked religious awe, and the stone itself must generally have become an object of worship. It is certainly remarkable that this origin was ascribed to several of the holy stones of antiquity.

The Diana of the Ephesians of the Acts of the Apostles, the “image that fell down from Jupiter” is perhaps the best known instance.

The Caaba, or black stone of Mecca, venerated by all Mohammedans, was worshipped by the Arabians in very early ages, and, although it has not been seen by any one specially qualified to judge, is now generally supposed to have been meteoric in origin. In Sale’s introduction to the Koran it is stated that this stone was supposed to have fallen down from heaven before the Deluge. Again Maximus Tyrius says that he had actually seen a quadrangular stone which was worshipped by the Arabians, and in the same passage he mentions that the Paphians worshipped a statue of Venus which looked like a white pyramid.

No doubt many of the holy stones were venerated on account of their form quite independently of their origin; the image of Venus in Cyprus is described by Tacitus as being not of human shape but conical; and he adds: "Et ratio in obscuro"; and Pausanias says that the images of Jupiter Melichius and of Diana were, the one a pyramid, and the other a column.

Even among the stones enumerated by Pliny which have been more or less identified with meteoric stones, the shape is one of the features according to which some at least were distinguished. The Ceraunia, or sky-stones, of his classification include as varieties stones which he refers to as Bætuli, Brontia and Notia, some of which have special shapes. All of these names frequently recur in mediæval literature.

It is evident that in one passage Pliny uses Ceraunia for a variety of precious stone, Beryl or Sapphire perhaps; but besides these he quotes Sotacus for the existence of two kinds of Ceraunia "which are black and red, resembling axes. Such as are black and round are holy things; cities and fleets can be captured by their means. A third sort greatly sought by the Magi are only found in places struck by lightning."

The word Bætylus remains a mystery; the name was primarily given to the stone which Saturn swallowed in mistake for Jove, but seems to have been subsequently applied to all meteoric stones. Hesychius suggests the Hebrew "Bethel" and the stone of Jacob as its origin; and this derivation seems to be accepted in the *Dictionary of the Bible*, though without any philological justification.

About the Brontia Pliny says that if we have sufficient faith we are to believe that they get into the heads of tortoises after thunderstorms; and here, I think, there is some confusion between the shape of some Brontia and the origin of others.

Through the midst of all this superstition, however, runs a continuous thread of reference to a celestial origin by which we are now able, in the light of subsequent experience, to trace a constantly recurring expression of the belief in meteoric falls.

The last statement for example in Pliny's enumeration appears to refer to meteorites ; but the remark about axes may indicate that stone celts or hammer-heads are denoted by his first class. The Cambridge authority, King, compares the German word *Donnerkeil* for Thunderbolt ; and again with the word *Bætuli* the Saxon "Beetle" which means a mallet, and concludes that these names in general refer to stone implements.

And here we are confronted by a curious complication in the history of the subject. Side by side with the fact that stones fell from the sky, existed the belief that the origin of a thing was indicated by its shape ; consequently a celestial origin was ascribed to those stones whose shape resembled that of a missile, and both stony concretions, fossils such as echini, and stone celts were supposed to be meteorites. It is difficult now to disentangle the evidence of falls actually witnessed from that which is merely based upon the shape of the stones to which many of the mediæval accounts relate.

At the present day both Belemnites and the marcasite nodules found in the chalk are popularly supposed to be thunderbolts on account of their shape.

Conrad Gesner in his book *De Figuris Lapidum* (1565) describes the various stones which derive their names from their real or supposed meteoric origin, the Ceraunias, the Chelonitis, the Brontias, the Bætylus, and gives figures of many. Some of these are obviously fossils, others are stone implements ; his accurate description of some which he had received as thunderstones from Kentman shows that they are clearly the latter. But it is equally certain that some of his words relate to real meteoric stones. He makes in particular this interesting remark : "The stone which fell from the sky in 1492 and is hung in the Church at Ensishheim and weighs 300 pounds (unless it has lost weight owing to the many visitors who take away fragments of it) has, I think, no particular shape" ; and he mentions that he had actually received a piece of this stone.

So much for the general evidence available about 300 years ago ; the last reference brings us to a time when stones

fell which are actually preserved at the present day, so that the veracity of contemporary accounts relating to them can no longer be questioned. From the sixteenth century onwards there are a number of such accounts in which we can now, reading by the light of subsequent experience, see internal evidence of their accuracy, and by which we are led to attach equal confidence to the accuracy of some of the earlier reports such as that of the *Ægos Potami* fall. Omitting, therefore, a number of mediæval references which may be found in the Saxon chronicles, Eusebius, Cardanus, Avicenna, Scaliger and others, we may pass directly to the Ensisheim fall, the earliest one of which we possess a contemporary account relating to a stone that still exists and has been proved to be meteoric.

Fall of the Ensisheim Stone.

The account is as follows :—

“On the 16th of November, 1492, a singular miracle took place. Between 11 and 12 in the forenoon with a loud crash of thunder and a prolonged noise heard afar off there fell in the town of Ensisheim a stone weighing 260 pounds. It was seen by a child to strike the ground in a field where it made a hole more than five feet deep. It was taken to the church as a miraculous object. The noise was heard so distinctly at Lucerne and many other places that in each of them it was thought that some houses had fallen. King Maximilian, who was then at Ensisheim, had the stone carried to the castle; after breaking off two pieces, one for the Duke of Austria and the other for himself, he forbade further damage, and ordered the stone to be suspended in the parish church.”

With this may be compared an account quoted by Sir Norman Lockyer from a rare tract in the British Museum, in which the obviously truthful statement of the occurrence is somewhat obscured by the fancy begotten by terror.

The tract is entitled :—

Looke up and see wonders: a miraculous Apparition in the Ayre, lately seen in Barkeshire at Bawlkin Greene neare Hatford. And is as follows :—

“ At Hatford some 8 m. from Oxford. Over this towne upon Wensday being the 9th of this instant Moneth of April, 1628, about 5 of the clocke in the after noone this miraculous, prodigious and fearefull handyworke of God was presented. A gentle gale of Wind then blowing from between the W. and N.W. in an instant was heard first a hideous rumbling in the Ayre, and presently after followed a strange and feare-full peal of Thunder running up and downe these parts of the countrey, but it strake with the loudest violence and more furious tearing of the Ayre about a place called the White Horse Hill. The whole order of this thunder carried a kind of majesticall state with it, for it maintayned (to the affrighted Beholder's seeming) the fashion of a fought Battaile. It began thus:—First for an onset went off one great Cannon as it were of thunder above like a warning peece to the rest that were to follow. Then a little while after was heard a second; and so by degrees a third untill the number of 20 was discharged in very good order though in very great terror. In some little distance of time after this was audibly heard the sound of a Drum beating a Retreate. Amongst all these angry peales shot off from Heaven this begat a wonderful admiration that at the end of the report of every cracke or Cannon-thundering, a hizzing noise made way through the ayre not unlike the flying of bullets from the mouthes of Great Ordnance; and by the judgment of all the terror stricken witnesses they were Thunder bolts. For one of them was seene by many people to fall at a place called Bawlkin Greene being a mile and a half from Hatford; which Thunder bolt was by one Mistris Greene caused to be digged out of the ground she being an eye-witnesse amongst many other of the manner of falling. The form of the stone is three-square and picked in the end: The colour outwardly blackish, somewhat like Iron; crusted over with that blacknesse about the thicknesse of a shilling. Within it is a soft, of a gray colour, mixed with some kind of mineral shining like small peeces of glasse.”

With this may further be compared the record relating to a fall of iron at about the same date (1620) but in a very

different part of the world. The following is a translation by Colonel Kirkpatrick from a contemporary Persian account of which he possessed the manuscript written by the Emperor Jehangire himself.

Fall of a Persian Meteorite.

“ Early on the 30th of Furverdeen, of the present year, and in the Eastern quarter of the heavens there arose in one of the villages of the Purgunnah of Jalindher, such a great and tremendous noise as had nearly, by its dreadful nature, deprived the inhabitants of the place of their senses. During this noise a luminous body was observed to fall from above on the earth, suggesting to the beholders the idea that the firmament was raining fire. In a short time the noise having subsided, and the inhabitants having recovered from their alarm, a courier was dispatched by them to Mahommed Syeed, the Aumil of the aforesaid Purgunnah, to advertise him of this event. The Aumil, instantly mounting his horse, proceeded to the spot where the luminous body had fallen. Here he perceived the earth, to the extent of ten or twelve guz in length and breadth, to be burnt to such a degree that not the least trace of verdure or a blade of grass remained ; nor had the heat which had been communicated to it yet subsided entirely.

“ Mahommed Syeed hereupon directed the aforesaid space of ground to be dug up ; when, the deeper it was dug, the greater was the heat of it found to be. At length a lump of iron made its appearance, the heat of which was so violent that one might have supposed it to have been taken from a furnace. After some time it became cold ; when the Aumil conveyed it to his own habitation, from whence he afterwards dispatched it in a sealed bag to court.

“ Here I had this substance weighed in my presence. Its weight was 160 tolahs. I committed it to a skilful artisan, with orders to make of it a sabre, a knife, and a dagger. The workmen soon reported that the substance was *not malleable, but shivered into pieces under the hammer*. Upon this, I ordered it to be mixed with other iron.

“ Conformably to my orders, three parts of the *iron of*

lightning were mixed with one part of common iron ; and from the mixture were made two sabres, one knife and one dagger. By the addition of the common iron, the new substance acquired a fine temper ; the blade fabricated from it proving as elastic as the most genuine blades of Ullmanny, and of the South, and bending, like them, without leaving any mark of the bend. I had them tried in my presence and found them cut excellently, as well indeed as the best genuine sabres. One of these blades I named *Katai* or *the cutter* ; and the other *Burk-serisht* or *the lightning natured*.

“ A poet composed and presented to me on this occasion the following tetrastich :—

This earth has attained order and regularity through the Emperor Jehangire :
In his time fell *raw* iron from lightning :
That iron was, by his world-subduing authority
Converted into a dagger, a knife, and two sabres.”

With these early examples of the more modern and authentic records may be compared the two following which are quite modern ; one relating to a meteoric stone that fell in Russia, and the other to an iron that fell in Mexico. The first has a special interest as the stone in which Diamond was found, and the second as the only modern meteorite which has been known to fall during a shower of shooting stars.

Fall of the Novo-Urei Stone.

“ At 7·18 A.M. on 22nd of September, 1886, some peasants were working in a field at Novo-Urei in Russia.

“ It was a dull morning without rain, although the sky was covered with clouds. Suddenly the air seemed filled with a bright light, followed in a few seconds by a violent report which was immediately succeeded by a second explosion. At the same moment the terrified peasants saw a fiery ball fall to the ground only a few yards from where they stood, and a second, but larger one was seen to descend into a neighbouring wood. The whole thing lasted less than a minute. The men fell in mortal terror to the ground and for some time dared not move. They thought that a frightful storm had burst over their heads,

and that fiery thunderbolts were falling. At length they recovered courage and went to the place where the thunderbolt had fallen. To their amazement they found here, in a small cavity a black stone half embedded in the earth, and still hot. It felt very heavy. They searched in vain for the other stone in the wood; but the next day a similar stone was found in a neighbouring field."

Fall of the Mazapil Iron, 1885.

"It was about nine in the evening when I went to the corral to feed the horses, when suddenly I heard a loud hissing noise exactly as though something red-hot was being plunged into cold water, and almost instantly there followed a somewhat loud thud. At once the corral was covered with a phosphorescent light, and suspended in the air were small luminous sparks as though from a rocket. I had not recovered from my surprise when I saw this luminous air disappear, and there remained on the ground only such a light as is made when a match is rubbed. A number of people from the neighbouring houses came running towards me, and they assisted me to quiet the horses which had become very much excited. We all asked each other what could be the matter, and we were afraid to walk to the corral for fear of being burned. When in a few moments we had recovered from our surprise we saw the phosphorescent light disappear, little by little, and when we had brought lights to look for the cause, we found a hole in the ground and in it a ball of fire. We retired to a distance fearing it would explode and harm us. Looking up to the sky we saw from time to time exhalations or stars which soon went out, but without noise. We returned after a little and found in a hole a hot stone which we could barely handle, which on the next day we saw looked like a piece of iron. All night it rained stars, but we saw none fall to the ground, as they seemed to be extinguished while still very high up."

But it is not necessary to multiply instances. It is clear that in ancient times and in the middle ages meteoric falls were often recorded and were implicitly believed by ordinary

people. Boetius de Boot in his book on Stones (1609) says : “ Si quis hanc vulgi opinionem refellere velit insipiens videatur ”.

The preceding examples will serve as a sketch of the evidence which presented itself to scientific men in the last century.

Meanwhile, however—and this is the fact to which I wish particularly to draw attention because it makes the history of meteorites so curious as a study of scientific evidence—the whole subject had with the growth of scientific knowledge become gradually discredited among thoughtful and well-educated people. Now that we know the fact to have been true, it is easy on the one hand to make allowances for the fancy which enters so largely into such past accounts as that of the Hatford fall, and on the other to reject among the present records which appear from time to time in the public press those which describe the fall of stones during thunderstorms, and under other improbable or impossible conditions, as well as the details imputed by terror and superstition.

But before the fact was known to be true, the evidence was so vitiated by delusions of various sorts, and eye-witnesses were so apt to be deceived by the sudden nature of the event and the terror which it inspired, that those who were best able to criticise circumstantial evidence were the first to reject that relating to meteorites.

I rather suspect that this was also so among the ancients, although the same critical attitude towards such events would hardly be expected from them. Aristotle barely alludes to thunderstones ; there appears to be no mention of them in Herodotus ; and Lucretius only asks why a bolt never falls when the sky is unclouded.

In later times neither Locke, nor Bacon, nor Newton appears to make any reference to the matter ; and Boyle only mentions meteorites as “ Stones which pass among the vulgar for thunderstones ”.

At the end of the last century the leaders of scientific thought had criticised the evidence and rejected it *in toto*.

Their position is really very well expressed more than

a century before by Torbernus Bergmann, the celebrated Professor of Chemistry at Upsala, in his treatise *De Avertendo Fulmine* (1764), where he makes the following observation: "Popularis erat veterum Teutonum Suionumque opinio lapides quosdam de coelo mitti, quos Thors-vigger (Donnerkeile, *i.e.* Lapides Ceraunios s. Belemnitas) vocabant;" and then he states that three opinions concerning these Ceraunian stones are held among philosophers. (1) That the whole thing is a fable, and that the stones themselves are weapons in which the handiwork of man is clearly apparent; (2) that these stones really fell to the ground with the lightning, as is thought by the Arabians; in which case they may either have been carried into the air by the wind, or may have been generated in the air as is suggested by Cartesius; or (3) that they have been fused into a mass at the point where lightning has struck the ground; an argument adduced in favour of this view by Stahlius is that a certain man, expert in such matters, having found a little hole in the ground while he was digging predicted that there would be a ceraunian stone at the bottom; which proved to be the case.

Bergmann himself rejects the first two hypotheses as clearly absurd; but being convinced by the recent discoveries of Franklin that the phenomenon is electrical, thinks that the last explanation is not only possible but probable.

It is rather difficult now to realise the attitude of mind adopted by the leaders of thought at the beginning of the present century. There was no lack of evidence; plenty of witnesses asserted that they had seen the stones fall, and many of them were actually preserved. Shooting stars have of course always been familiar, just as they are at the present time, but the scientific authorities of that date after duly weighing the evidence came to the conclusion that there was no proof that these stars ever fell to the ground. They preferred to believe that those who professed to have witnessed such falls were mistaken, and that the supposed meteorites were ordinary stones struck by lightning. In fact, the witnesses generally mentioned thunder and lightning as accompanying the fall; this in itself was

suspicious ; and, further, the witnesses were evidently so scared that they hardly knew what they had seen. And yet one cannot help feeling that the available evidence, if acutely criticised, was sufficient to enable a scientific critic to extract the truth from the mass of legend in which it was embedded ; and in fact this was actually done with signal success by a writer whose work opens the last chapter in the history of the belief in meteorites.

The modern development of a scientific proof of the existence of sky-stones, as distinct from terrestrial material is no doubt familiar to many through Mr. Fletcher's admirable *Introduction to the Study of Meteorites*. At the risk of considerable repetition I must give a brief sketch of the meteoric events of the last decade of the eighteenth and the first decade of the nineteenth century, with the object of showing how the evidence was received by the critics of that date, and how they were finally persuaded. The chapter of proof really begins in the year 1794, when the German physicist Chladni wrote a very remarkable paper, "Ueber den Ursprung der von Pallas gefundenen und anderer ihr ähnlicher Eisenmassen".

The traveller Pallas in 1772 saw in Siberia a great mass of iron weighing about 1500 pounds which had been discovered by a Cossack at the top of a mountain near Krasnojarsk in Siberia. It was spoken of by the Tartars as a holy thing fallen from heaven. There was nothing like it in the neighbourhood and it was too large to have been transported to the mountain top by human agency. It was a peculiar spongy-looking mass which strongly recalls Pliny's description quoted above.

Chladni argued that this iron had evidently been fused, but not by man, electricity, or accidental fire, considering the place where it was found ; there are no volcanoes anywhere in the neighbourhood ; therefore it must have fallen from the sky. To the same origin he referred a huge mass found by Indians at Otumpa far away in the Argentine Desert of South America ; a mass which was at first supposed to be an iron mine ; and he suggested that other masses of native iron are also meteoric.

Chladni even went so far as to suggest that these masses were bodies of the same sort as those which produce the appearance of a shooting star in their passage through the air.

He subsequently fortified his views by an enumeration of a great number of reported falls of stone from the sky in ancient and mediæval times, of which I have quoted several above.

Of course Chladni's theory was not accepted—it was so improbable, and his arguments seemed to be only based upon the difficulty of accounting for the presence of this particular mass of iron in Siberia in any other way. His contemporaries regarded the essay as an ingenious but unconvincing piece of work.

Fall of the Sienna Stone.

Immediately after the appearance of Chladni's paper, however, a remarkable event took place at Sienna in Tuscany on 16th June, 1794, at 7 o'clock in the evening.

The event is thus described in the following letter from the Earl of Bristol to Sir William Hamilton which has been often quoted.

“In the midst of a most violent thunderstorm about a dozen stones of various weights and dimensions fell at the feet of different persons, men, women and children. The stones are of a quality not found in any part of the Siennese territory ; they fell about eighteen hours after the enormous eruption of Mount Vesuvius ; which circumstance leaves a choice of difficulties in the solution of this extraordinary phenomenon. Either these stones have been generated in this igneous mass of clouds which produced such unusual thunder ; or—which is equally incredible—they were thrown from Vesuvius at a distance of at least 250 miles : judge then of its parabola. The philosophers here incline to the first solution. I wish much, sir, to know your sentiments. My first objection was to the fact itself, but of this there are so many eye-witnesses it seems impossible to withstand their evidence.”

Sir Wm. Hamilton (*Phil. Trans.*, 85, p. 103), after quoting this letter says :—

“The outside of every stone that has been found, and

has been ascertained to have fallen from the cloud near Sienna, is evidently freshly vitrified, and is black, having every sign of having passed through an extreme heat ; when broken, the inside is of a light grey colour mixed with black spots, and some shining particles, which the learned here have decided to be pyrites, and therefore it cannot be a lava, or they would have been decomposed. Stones of the same nature, at least as far as the eye can judge of them, are frequently found on Mount Vesuvius ; and when I was on the mountain lately, I searched for such stones near the new mouths, but as the soil round them has been covered with a thick bed of fine ashes, whatever was thrown up during the force of the eruption lies buried under those ashes. Should we find similar stones with the same vitrified coat on them on Mount Vesuvius, as I told Lord Bristol in my answer to his letter, the question would be decided in favour of Vesuvius ; unless it could be proved that there had been, about the time of the fall of these stones in the Sanese territory, some nearer opening of the earth, attended with an emission of volcanic matter, which might very well be, as the mountain of Radicofani, within fifty miles of Sienna, is certainly volcanic. I mentioned to his lordship another idea that struck me. As we have proofs during the late eruption of a quantity of ashes of Vesuvius having been carried to a greater distance than where the stones fell in the Sanese territory, and mixing with a stormy cloud have been collected together just as hailstones are sometimes into lumps of ice, in which shape they fall, and might not the exterior vitrification of those lumps of accumulated and hardened volcanic matter have been occasioned by the action of the electric fluid on them ? The celebrated Father Ambrogio Soldoni, professor of mathematics in the university of Sienna, is printing there a dissertation upon this extraordinary phenomenon, wherein, as I have been assured, he has decided that those stones were generated in the air, independently of volcanic assistance."

Soldoni's account contains the following additional details : "Two ladies being at Coyone, about twenty miles from Sienna, saw a number of stones fall with a great noise

in a neighbouring meadow ; one of which, being soon after taken up by a young woman, burnt her hand ; another burnt a countryman's hat ; and a third was said to strike off the branch of a mulberry tree, and to cause the tree to wither ”

Soldoni himself thought that “ the stones were generated in the air by a combination of mineral substances which had risen somewhere or other as exhalations from the earth, but not from Vesuvius ”.

Very shortly afterwards (1796) appeared the work of Edward King, *Remarks Concerning Stones said to have Fallen from the Clouds*, in which this and other falls were enumerated and discussed. In regard to the Sienna stones he recalls instances in which volcanic dust was known to fall upon ships 100 leagues from the scene of eruption, and quotes Sir William Hamilton's account of the Vesuvius eruption in which ashes appeared to be projected to a height of twenty-five or thirty miles ; he suggests as an explanation of the Sienna stones that these ashes were carried beyond Sienna northwards, and were then brought back by a northerly wind, congealing from the air, which he had always regarded as “ the great consolidating fluid out of which all solid bodies are composed ”.

Fall of the Wold Cottage Stones.

At the very time when King was writing, a stone was being exhibited in London which weighed fifty-six pounds and was seen to fall at Wold Cottage in Yorkshire on 13th December, 1795.

The following is the account given by the handbill which accompanied the exhibition : “ It penetrated through twelve inches of soil and six inches of solid chalk rock, and in burying itself had thrown up an immense quantity of earth to a great distance ; as it fell a number of explosions were heard about as loud as pistols.

“ In the adjacent villages the sounds heard were taken for guns at sea ; but at two adjoining villages were so distinct of something passing through the air towards the habitation of Mr. Topham that five or six people came up to see if anything extraordinary had happened to his house or

grounds. When the stone was extracted it was warm, smoked, and smelt very strong of sulphur. Its course, as far as could be collected from different accounts was from south-west. The day was mild and hazy; the sort of things very frequent in the Wold Hills where there are no winds or storms; but there was not any thunder or lightning the whole day. No such stone is known in the country. There was no eruption in the earth: and from its form it could not come from any building, and as the day was not tempestuous it did not seem possible that it could have been forced from any rocks, the nearest of which are those of Flamborough Head, a distance of twelve miles. The nearest volcano I believe to be Hecla in Iceland."

It might be thought that an examination of the stones themselves would be sufficient to prove or to disprove the common belief about their origin; and about this time an examination of the sort was undertaken by some of the leading French chemists, who actually made an analysis of the Ensisheim stone, and, finding it to contain nothing new, concluded that it was terrestrial. Their report on these supposed sky-stones terminated with the words: "Ignorance and superstition have attributed to them a miraculous existence at variance with the first notions of natural philosophy".

Fall of the Benares Stone.

In the year 1798, another well-authenticated fall took place in India, fourteen miles from Benares, where a luminous meteor was observed in the western heavens at 8 P.M. accompanied by a loud noise resembling thunder. The sky was perfectly serene; not the smallest vestige of a cloud had been seen for about eight days, nor were any seen for many days after. "Inhabitants observed that the light and thunder were accompanied by the noise of heavy bodies falling. Uncertain whether some of their deities might not have been concerned in this occurrence they did not venture out to inquire into it until the next morning, when the first circumstance which attracted their attention was the appearance of the earth being turned up

in different parts of their fields, where on examining they found the stones."

Again in the same year a fall was reported at Villefranche, near Lyons; the meteor was seen by many people and the eye-witnesses were horribly alarmed. One man whose house was within twenty paces of the spot where the stone fell was so terrified by the noise that he "shut himself up with his family in the cellar, and then in the bed-chamber, where, fear prevailing over curiosity, he spent the night without daring to go out to examine what had happened".

By this time Chladni's memoir had attracted attention to at any rate the possibility of the truth of such reports, and all these recent occurrences gave rise to much discussion. It will be sufficient to quote a few of the contemporary criticisms in order to gain some idea of the prevailing impression which they created among those who read them.

W. Beauford writing in the *Philosophical Magazine* in 1802, concludes that the matter must be of volcanic origin and derived either from Vesuvius, Etna, or Hecla. But the distances are too far for them to have traversed as stones. "Hence, if they originate from volcanic ashes they must be formed in the clouds where those ashes meeting with carbonic, sulphuric and other acids, and mixing with earthy particles drawn from terrestrial objects are by the electric fluid in the lightning precipitated from the aqueous vapours which bore them up, and, becoming united, fall to the earth in the form of stones, as in some measure is evinced from the flashes of light and detonation which accompany their fall."

Pictet writing on behalf of the French National Institute in 1803 expressed the opinion that "the attention of philosophers should be directed to the subject in order that the phenomenon if true may be confirmed—or if only an illusion supported by popular error may be consigned for ever to the class of errors". In the same year the French Institute mentions new motives to "induce philosophers to examine and appreciate the different testimonies in consequence of which the stones in question have been supposed to have fallen from the clouds. When a phenomenon

is announced if we were able to ascertain by a complete enumeration of the different physical agents that none of them is capable of producing it the impossibility of the phenomenon would be the inevitable result and consequently the falsity of the account. But on the other hand, when we find a cause which establishes the possibility of it, if sound logic forbids us to ascribe it exclusively to this cause, it commands us at the same time to substitute doubt for complete negation and to employ every means possible of confirming the fact, because it is not repugnant to the general laws of Nature."

This very guarded and somewhat curious statement is explained by the fact that Laplace and Poisson had calculated that a body projected from the moon would require only a velocity five times as great as that of a bullet of a twenty-four pounder, discharged with a quantity of gunpowder equal to half its own weight, to reach the earth after a journey of sixty-four hours, and would arrive with a velocity of 31,000 feet a second. It is evident that the accounts of the falls themselves were by this time no longer discredited, and that even the lightning theory was losing its adherents.

In 1803 Olbers, who had at first asserted that the Sienna stones were from Vesuvius, is led by the similarity of the sky-stones in different parts of the world to agree that they had a common origin and probably came from the moon. The chemist Vauquelin also inclined to the moon theory; it is evident that the absence of atmosphere there would account for the stones leaving a lunar volcano without retardation and also without experiencing oxidation. Writing of the Barbotan fall which took place in 1789 he says: "Some peasants brought stones which they said were the result of the fall of the meteor; but at that period they were laughed at. What they said was considered as fables—and those to whom the stones were offered would not accept of them. The peasants would now have more reason to laugh at the philosophers."

Even at this period, however, when it began to be suspected that stones really fell from the sky and that they may have a common origin, it was by no means universally conceded that they were extraterrestrial.

Proust, in a paper published in the *Journal de Physique* in 1805 (reported in *Nicholson's Journal*, vol. xii.), describes a stone which fell in 1773 at Sena in the district of Sigena, in Spain; and gives the results of an analysis. He concludes that such stones "cannot subsist in any of the habitable parts of the globe. But from the eternal cold of the polar regions, where water remains for ever a solid mass, and iron cannot rust, we may reasonably look to these regions as the native place of such bodies."

But we can now hurry to the close of the story.

It is pretty evident from the preceding quotations that at the beginning of the present century the attitude of scientific men towards the reported fall of meteorites was one of suspicious indifference. There might be something in it all; there was fair evidence in many cases that something startling had happened; but no reliance could be placed upon the evidence of the senses under such conditions; and the witnesses were generally ignorant rustics.

It had been proved by Franklin that lightning is the same as the electric spark; and thunder is an accompaniment of lightning. The witnesses of these events professed to have heard thunder; what they saw and found were, no doubt, ordinary stones struck by lightning; and this conclusion seemed to be supported by chemical and mineralogical study of the stones themselves.

In the meantime an English chemist was, unnoticed, pursuing the only satisfactory method of completing the scientific proof which had been initiated by Chladni's acute reasoning.

This chemist, Edward Howard by name, collected pieces of four stones, those which fell at Sienna, Wold Cottage, Benares, and one which fell during a thunder-storm in 1753 in Bohemia. He made analyses of them and submitted them for mineralogical investigation to the Count de Bournon.

The results of his long and patient investigation were communicated to the Royal Society in 1803. He concluded that all these four stones had nearly the same chemical composition; and that though there was nothing actually new in them, their mineral composition was so unlike that of all terrestrial stones, and so similar for the four

masses—though they came from widely distant places and were asserted to have fallen at very different dates—that they must have had a common origin ; and he concluded, though with diffidence, that they may very possibly be really meteoric.

This paper attracted much attention in the scientific world, and the opportunity for putting it to the test soon occurred in France, where the new views met with the greatest opposition. A shower of stones fell on 26th April, 1803, at L'Aigle in the department of Orne. The eminent physicist Biot was sent down by the French Academy to investigate the matter, and reported that there was no doubt that a violent explosion was heard that day for seventy-five miles round ; that a fire ball was seen, though the sky was clear ; and that about 3000 stones fell within a space of six by two miles.

From this time the fall of meteorites was no longer doubted. The subsequent discoveries and the present state of our knowledge are admirably stated in Fletcher's *Introduction* referred to above, and can be further pursued in the special treatises on the subject.

On a review of the whole story one cannot help feeling that although the scientific proof could never have been complete without the work of Howard, and that his work was of an extraordinarily difficult nature, as is proved by its previous failure in the hands of the French chemists, yet the arguments of Chladni might have been advanced at almost any previous period had some sufficiently acute critic cared to examine the evidence without prejudice. The history traced in the foregoing pages is a curious study of the rejection of circumstantial evidence owing to its surprising nature and to the superstition with which it was mixed. The fault lay, as is clear from the official statement of the French Institute, in the refusal to accept the evidence relating to a phenomenon for which a sufficient cause could not be at once suggested—a very common but a very dangerous attitude. Doubtless our successors will be able to regard with equal curiosity either the prejudice or the credulity with which many a problem is regarded at the present day.

H. A. MIERS.

METABOLISM OF THE SALMON.

“THE curious life history of the salmon has always been a subject of the deepest interest not only to the zoologist and physiologist, but also to the sportsman. In spite of the most careful study by scientific investigators, the migrations of the salmon and the various changes in condition that it undergoes are even now far from being fully understood ; and the careless observations and foolish traditions of keepers, fishermen and gillies have only served to involve the matter in a deeper cloud of mystery.”

The foregoing is the opening sentence of a report¹ which has recently been presented to the Fishery Board for Scotland by Dr. Noël Paton. In order to dispel this veil of ignorance, Dr. Paton has undertaken an extended series of observations, which have been carried out in the Research Laboratory of the Royal College of Physicians of Edinburgh. The subdivisions of this subject are so numerous, and the points to be investigated so diverse that Dr. Paton has adopted the wise measure of obtaining the co-operation of several other workers in the laboratory. This union of forces has produced a result which would have been beyond the power of any individual investigator.

The principal subjects of the research consist in a verification of the alleged abstention from food which the fish exercises when in fresh water, the details concerning the growth of the generative organs which occurs during this period, the simultaneous decrease in the muscular tissue, and the consequent deterioration of the food value of the fish, a discussion on the sources of muscular energy, and of the metabolic exchanges in fats, proteids, iron, phosphorus, pigments and so forth.

Such an enumeration of the chief subjects treated will indicate the wide scope of the work ; in fact it forms the

¹ *Report of Investigations on the Life History of the Salmon*, edited by D. Noël Paton, 1898. See also *Journal of Physiology*, 1898, vol. xxii., p. 333.

most important contribution to the subject which has appeared since the publication of Miescher-Ruesch's writings. Some of the outcomes of Miescher's work have already appeared in this Journal¹ in papers by Dr. Brodie and Mr. Escombe, though neither of these papers was written with special reference to the salmon.

Miescher's observations were made on Rhine salmon, and the principal conclusions he drew were that the fish does not feed during its sojourn in fresh water, that the fat and proteid stored in the muscles is transferred to the growing ovaries and testes, but that the material stored in the muscular tissue is more than sufficient for that purpose.

It was to fill up numerous gaps in these observations that Dr. Paton undertook the large task he has so successfully completed, and every page of his full report is worth the careful study of physiologists. All I am able to attempt here is a brief summary of his principal conclusions.

Before doing this, however, let me enumerate some of the subjects of biological and economic interest which were not investigated. This I may best do by another quotation from the concluding sentences of the report: "As regards the course of migration, our investigations cover only a few months of the year, and interesting results are to be expected by extending the investigations into other seasons. Whether the *rate of migration* can be satisfactorily ascertained in our short Scottish rivers is very doubtful. In the great Canadian rivers, such as the Fraser, very valuable results might be expected from a study of this question. Indeed it would be a matter of the greatest importance to have the observations recorded in these papers checked and extended on a large scale in such a river, with its unbounded supply of fish and hundreds of miles of waterway. . . .

"The downward migration of kelts (the young fish) requires further study. Of the twenty-two kelts received in April, 1897, all were females. Is this a mere accident,

¹ SCIENCE PROGRESS, April, 1898, "The Phosphorus Containing Substances of the Cell," by T. G. Brodie; "Germination of Seeds," by F. Escombe.

or do the male kelts descend at a different time? The interesting question of the loss of the great maxillary development in the male is also yet to be elucidated."

May I be permitted to re-echo this hint to our Canadian cousins? Those of us who last year had the opportunity of experiencing Canadian hospitality will know the favour with which science is regarded in the Dominion by people and Government alike; and of the many wonderful sights those who travelled to the West were privileged to witness, that of the salmon in the rivers of British Columbia will probably be the one which will most vividly imprint itself on the memory. We hope that some scientific use may be made of such opportunities.

Turning, however, to the body of the report, let me now briefly indicate the principal lines of research contained in it.

The first question: Do salmon feed while in fresh water? was taken up by Drs. Gulland and Gillespie. It seemed probable that a conclusive answer might be arrived at by three different lines of investigation, *viz.* :—

(1) The condition of the mucous membrane of the alimentary canal.

(2) The activity of the digestive secretions.

(3) The bacteriology of the alimentary tract.

In connection with the first of these questions, the inactive condition of the glandular epithelium, which was in great measure desquamating, the absence of zymogen granules in the pancreas, the fatty condition of the liver, the emptiness of the gall bladder, and the absence of even a trace of food, all point conclusively to the inactivity of the alimentary canal. This conclusion is supported by what was found in connection with the second question, namely, that the proteolytic and diastatic action of extracts of various parts was extremely low. If, for instance, in the case of the stomach, the peptic activity be expressed in the case of kelts as 30, that of fish from the estuaries was 9.5, and from the upper waters, 9.1. The digestive activity was proportional to the acidity of the glycerine extract, and this in turn was in inverse proportion to the number of micro-

organisms. The acid present is organic in nature, and hydrochloric acid is absent. The number of bacteria in the tract is very great, especially during the warm summer months; this fact is opposed to Miescher's idea that putrefaction does not occur so readily in the upper water fish as in those from the estuaries. The increase in organisms is probably due to the diminished acidity of the gastric contents.

In making the observations bearing on metabolism, Dr. Paton is himself principally responsible, though some of the details were left to his colleagues, Dr. J. C. Dunlop, Mr. Mahalanobis, Dr. F. D. Boyd and Dr. E. D. W. Greig.

Salmon were procured from the estuaries and from the upper reaches of the three rivers, Helmsdale, Spey and Dee, from May to November. It was not possible to say when the fish captured in the upper waters in May and June had left the sea, and it was considered fairest to limit the investigation to a comparison of the upper water fish of July and August onwards with the estuary fish from May onwards. The whole fish in each case was available for analysis; in the muscles, separate analyses were made of both "thick" and "thin" portions of the musculature, and for tabular purposes, the figures are in all cases reduced to a fish of standard length, *viz.*, 100 cm., the length of a salmon of about 30 lb.

The tables of analyses given bear witness to the loss of total solids in the muscles, and the gain in the weight of ovaries or testes as the case might be. The large difference between these two numbers gives the amount available as a source of energy.

Among the solids particular attention was directed first to the fats, and secondly to the proteids. The fat stored round the pyloric appendages and in the liver is also taken into account. The numbers are much more complete with regard to female fish than male fish, the number of male fish available being too small for the drawing of satisfactory averages.

The changes in the muscle are described by Miescher-

Ruesch as a degeneration, but this conception of the process is not supported by histological examination. What occurs is an accumulation of fat outside and within the fibres while the fish is feeding, and this is subsequently, during the inanition period, used up for the construction of the genitalia and as a source of muscular energy.

The "curd" of salmon muscle which is so marked in the fish just leaving the sea early in the year is composed of proteids and fats.

The nature of the proteids in the muscles was investigated by Dr. Dunlop. He finds paramyosinogen, myosinogen, and myoglobulin which are all soluble in salt solution; a nucleo-proteid corresponding to that found by Pekelharing, and called myostromin by Karajew; this is partly soluble in salt solution, and wholly so in 1 per cent. caustic soda solution. Collagen from the interstitial connective tissue was not dissolved by either reagent. True albumin was not found, and proteoses and peptose were also absent. No mention is made of what v. Fürth calls myo-proteid, and which he considers to be characteristic of fishes muscle, nor of Siegfried's nucleon. It is apparently the soluble proteids which undergo the diminution demonstrated to occur as the fish proceeds to the upper waters.

Coming now to the summary of metabolic exchanges in fat and proteid, we enter the region of controversy; and in view of the recent discussion by Pflüger and others of the part played by fats and proteids respectively as a source of muscular energy, the present observations on a cold-blooded animal in which the decomposition of the fats can be only sparingly connected with the evolution of heat, are of no little interest.

If the fat and proteid going to the ovaries and testes is subtracted from that lost from the muscles, the residue gives the amount available for the liberation of energy. The absolute numbers are given in the report, but it will be sufficient here to mention the proportion found. In female fish up to August the energy from proteids to energy from fats is as 1 : 4 · 2; extending the observations to November the proportion rises to 1 : 7 · 6. In male fish

up to August, the proportion is 1:11·6. Sufficient male fish in the later months were not obtainable.

In connection with phosphorus, the results indicate that the phosphorus stored in the muscles as simple phosphates is transferred to the ovaries and testes, and there built up into organic combinations. In both glands, lecithin appears to occupy an important step in this conversion; but while in the testes the change to the true nucleins is carried out at once, in the ovum an intermediate product ichthulin (a pseudo-nuclein) is first formed, and undergoes the change into nuclein as the embryo develops.

The gain of iron which occurs in the ovaries is not derived from the muscles nor from the liver. In all probability its source is therefore the hæmoglobin of the blood.

The last point I shall mention is that relating to the pigments, which were investigated by Miss M. I. Newbigin, who is already well known from her work on Crustacean pigments. The colour of salmon flesh is due to two lipochromes, one being the widely distributed yellow pigment lutein, the other a bright red pigment closely resembling that occurring in the Crustacea.

The same pigments are found in the ovaries, and as the season advances the red colouring matter accumulates in the ovaries and disappears from the muscles. The red lipochrome probably originates from the yellow, and the latter is probably derived from the herring, etc., on which the salmon feeds. The purpose which the pigment seems to serve is to assist in concealing the ova when they are shed.

The main conclusions to which the report points are the following: First, that when the fish enters the river it ceases to feed, and has to rely entirely on its own resources, namely, the materials stored in its muscular tissue. The evidence on which this rests is much more conclusive than in Miescher's earlier investigations; though perhaps the sceptical fisherman may still ask, Why, then, does the salmon rise to the fly?

Secondly, of the stored material by far the larger fraction is used for the development of kinetic energy; this is especially true for the fats, particularly in the latter portion

of the fish's stay in fresh water. A comparatively small fraction of the muscular store is transferred to the growing genitalia. The substances thus transferred are proteid, fat, phosphorus-containing materials, and pigment; the phosphorus containing materials undergo certain chemical changes in the transference, but serve chiefly in the synthesis of nuclein; it appears probable that the proteid and fat would undergo analogous intramolecular rearrangements also. Of the substances under investigation only one, the iron, is not derived from the muscle; this comes from the hæmoglobin of the blood.

W. D. HALLIBURTON.

THE PHYSIOLOGICAL EVOLUTION OF THE WARM-BLOODED ANIMAL.

IN physiological text-books it is customary to point out how widely warm- and cold-blooded animals differ from each other in the absolute temperature of their bodies, and in the manner of their reaction to change of external temperature. In practically no case, however, is an attempt made to connect the two classes of animals, and to show, by means of connecting links, how the one class may have been evolved from the other. Doubtless this was in part due to actual lack of data. But now, in consequence of the recent publication of a very interesting paper by Sutherland,¹ on the temperatures of monotremes and marsupials, it is possible to trace with more or less completeness the various stages by means of which the lower invertebrates may have been gradually evolved, in a physiological sense, to produce a warm-blooded animal, such as man, in which the nervous system appears to possess almost perfect power of keeping the temperature of the body constant, whatever be the temperature, or variations in the temperature, of the immediate environment.

It might be thought that, in their reaction to temperature, all cold-blooded animals are alike. But this is by no means the case. There has taken place among them a gradual evolution of the nervous control of the tissue metabolism, which is probably quite as great as that separating the typical warm-blooded animal from the higher cold-blooded one. There would even seem to be a gradual evolution in the reaction of tissue change to temperature in respect of the tissues themselves, apart from a special nervous controlling influence. Thus the writer,² as the result of observations on the respiratory activity of various marine invertebrate and vertebrate animals, at various temperatures, came to the conclusion that this was not by

¹ *Proc. Roy. Soc. Victoria*, vol. ix., p. 57, 1897.

² *J. Physiol.*, xix., p. 18, 1896.

any means equally affected in the different animals by equal variations of temperature. In the following table are given what were called the "temperature increments" of the animals experimented on, or the relations of the respiratory activity at 24° to the respiratory activity at 10°:—

Name of Animal.	Class or Order.	Temperature Increment.	Per cent. Solids in Tissues.
Beroë ovata - -	Ctenophora -	5·1	·60
Cestus veneris - -	"	4·4	·24
Salpa tilesii - -	Tunicata -	4·5	·43
Salpa pinnata - -	"	4·3	·26
Rhizostoma pulmo -	Scyphozoa -	3·7	·53
Carmarina hastata -	Hydrozoa -	2·2	·38
Pterotrachea coronata	Heteropoda -	3·2	·53
Octopus vulgaris -	Cephalopoda	2·5	11·7
Tethys laporina -	Gasteropoda -	2·0	1·20
Amphioxus lanceolatus	Acraniata -	2·7	12·8
Serranus scriba - -	Pisces -	2·6	16·7
Heliasis chromis -	"	1·9	22·3
		mean 4·8	mean ·42
		" 4·4	" ·35
		" 3·0	" ·46
		" 2·6	" 4·48
		" 2·7	" 12·8
		" 2·3	" 19·5

Here it will be seen that the increments vary from 5·1 to 1·9 ; or, if the various animals be grouped more or less according to their morphological relationships, and means taken, the numbers vary from 4·8 for the Ctenophores examined, to 2·3, or less than half the amount, for the teleost fish. With evolution of morphological structure, there would, in fact, appear to be a gradual evolution of increased power of resistance to variations of temperature. The tunicate *Salpæ* appear to form an exception, but, omitting them, we find that the temperature increment gradually diminishes as we pass first to the two medusæ examined, then to the three mollusca, then to the acraniate *Amphioxus*, and lastly to the teleost fish. As may be gathered from the last column of the table, this gradual evolution is accompanied by a gradual increase in the percentage of solids in the tissues of the organisms. Indeed, it seems highly probable that it is to this cause, rather than to the increased complexity of morphological structure, and growth of nervous control over the tissue metabolism, that the increased resistance to temperature changes is chiefly due. Thus all the animals in the above

table may be roughly divided into two classes. The first seven of them are transparent pelagic animals, in which the temperature increment is, with one exception, 3·2 and upwards, and the percentage of solids in the tissues ·6 per cent. or less. The remainder, also with one exception, *viz.*, *Tethys*, are littoral animals, and not transparent, have a temperature increment varying from 1·9 to 2·7, and contain from 11·7 to 22·3 per cent. of solids. In any case, whether the increased resistance to temperature change be ascribed to the one factor or the other, it is evident that there has taken place an evolution of this physiological characteristic, and that it has, at least to some extent, proceeded hand in hand with the morphological evolution.

In the above-mentioned animals, the respiratory activity appeared to increase regularly with the temperature, and there was no evidence that the nervous system had any special power of influencing the metabolism. It has also been hitherto generally considered that this is true for all cold-blooded animals, and that, provided these are in a state of rest, their metabolism and body temperature depend only on the temperature of their surroundings. As a matter of fact, the few data available did not altogether warrant this conclusion. Thus Moleschott¹ found the carbonic acid output of frogs at 5° to be in some cases greater than that at 10° and 15°. Also Schultz,² in his determinations of the carbonic acid discharge of the edible frog, *Rana esculenta*, found the metabolism at 14·4° to be less than that at 6·4°.

The writer³ has recently made similar determinations of the relations of the respiratory activity to temperature in various amphibia and other cold-blooded animals, and has found that in hardly any case does the carbonic acid output increase regularly with increase of temperature. There exist temperature intervals over which the metabolism remains either constant, or varies but slightly. The animals were slowly warmed from 2° to 30°, or slowly cooled from 30° to 2°, and the carbonic acid discharge determined over

¹ *Untersuchungen zur Naturlehre*, 1857, Bd. ii., 315.

² *Pflügers Archiv*, xiv., 78.

³ *J. Physiol.*, xvii., p. 277, 1894, and xxi., p. 443, 1897.

temperature intervals of 2.5° or 4° . By making several determinations with each animal, the average relation of respiratory activity to temperature could thus be fixed with a fair amount of accuracy. In the following table are given the temperature intervals, both on warming and on cooling, through which the carbonic acid output of the animals remained practically constant. In those cases in which the figures are placed in brackets the metabolism increased or decreased with increase or decrease of temperature, but to a much smaller extent than at temperatures above or below the limits mentioned.

Animal.	Temperature intervals of approximately constant CO_2 output.		CO_2 output at 24°
	On warming.	On cooling.	CO_2 output at 10°
English frog (<i>R. temp.</i>) (old series)	$6^{\circ}-17.5^{\circ}$	$(17.5^{\circ}-12.5^{\circ})$	} 2.3
„ „ (new series)	$12.5^{\circ}-15.0^{\circ}$	$(17.5^{\circ}-12.5^{\circ})$	
Edible frog (<i>R. esculenta</i>)	$15.0^{\circ}-20.0^{\circ}$	$(17.5^{\circ}-2^{\circ})$	2.1
Common toad - - -	$10^{\circ}-22.5^{\circ}$	$(17.5^{\circ}-2^{\circ})$	2.0
Axolotl - - -	$(2^{\circ}-20^{\circ})$	$(20^{\circ}-2^{\circ})$	2.2
Newt - - -	$10^{\circ}-22.5^{\circ}$	$25^{\circ}-17.5^{\circ}, 15^{\circ}-12.5^{\circ}$	1.4
Blindworm - - -	$(10^{\circ}-20^{\circ})$	$(20^{\circ}-2^{\circ})$	3.1
Snail - - -	$20^{\circ}-27.5^{\circ}$	$(17.5^{\circ}-15^{\circ})$	2.4
Earthworm - - -	$10^{\circ}-22.5^{\circ}$	$(20^{\circ}-10^{\circ})$	1.9
Cockroach - - -	—	—	4.3

In the case of the English frog, one series of determinations showed the metabolism to remain practically constant from 6° to 17.5° on gradually warming the animal, and to diminish only slightly between 17.5° and 12.5° on gradually cooling it. In another series, made three years later, the carbonic acid output remained constant for a considerably shorter interval on warming, but the rate of increase, with increase of temperature, was very much less at temperatures between 2° and 25° , than at temperatures above 25° . Of all the nine animals examined, the newt, the toad, and, strangely enough, the common earthworm, exhibited this constancy of carbonic acid output to the most marked extent. Thus in all of these animals, it remained practically

constant from 10° up to 22.5° on warming, and diminished only very slightly over similar temperature intervals on cooling. In the cockroach alone of the animals examined, did the carbonic acid output increase and decrease uniformly with increase and decrease of temperature.

To what is this want of dependence of metabolism on temperature due? It must obviously be ascribed either to a direct control of the central nervous system over the respiratory activity of the tissues, or to the tissues being themselves so constituted that their metabolism is affected by temperature in this curious and irregular manner. This second supposition appears on the face of it highly improbable, and that it is actually incorrect was experimentally proved. Thus it was found that if a transverse section were made in the medulla of the frog or toad, at the level of its lower border, the carbonic acid output now varied uniformly with the temperature and showed no intervals of constancy. If, however, the transverse section were made at the level of the upper border of the medulla, the relation of carbonic acid output to temperature was as a rule the same as in intact frogs, though occasionally irregular results were obtained. It appears therefore that there is a definite centre situated somewhere in the region of the medulla, and that this centre is able to send out impulses to the various tissues of the body, and so control their metabolism. Probably the muscles are the tissues chiefly concerned in the maintenance of this metabolism, and it might be thought that it is maintained through the agency of the ordinary motor nerves which are concerned in muscular contractions. This does not seem to be the case, however, as it was found that if doses of curare were given to frogs sufficient to paralyse all voluntary and reflex muscular movements, the carbonic acid output still varied irregularly with the temperature. It was only when excessive doses of the drug were administered that the one varied regularly with the other.

We see, therefore, that in almost all the cold-blooded animals examined, there exist intervals of temperature over which, through the agency of a nervous regulating mechanism, the metabolism remains more or less constant. When

so lowly an animal as the earthworm possesses the power of perfectly controlling its carbonic acid discharge, it is obvious that in many other members of the animal kingdom, thus far unexamined, the same condition of things may be present. It is noticeable that the intervals of constant metabolism more or less cover such variations of temperature as the animals would be ordinarily exposed to under normal conditions, except in the extreme cold of winter and extreme heat of summer. The teleological explanation of such an arrangement is obvious. If the metabolism depended absolutely on the temperature, the animal would lose much of its control over its own powers. It would of necessity be always lively in hot weather, and torpid in cold. All its movements would be regulated by the temperature of its surroundings, and not by its own wants and desires. In cold weather it could not move fast enough to escape its enemies, should they be warm-blooded ones, and in hot weather it would have difficulty in obtaining sufficient food to replace the increased tissue loss.

In the last column of the above table are given the temperature increments, or relation of the carbonic acid output at 24° to that at 10° . It will be seen that with one or two exceptions, which are probably in part accidental, these values remain at about 2.0, or about the same as those obtained in the former experiments on teleost fish. In the case of the cockroach, however, the very high value of 4.3 was obtained, or as great an effect as was noticed in the transparent pelagic coelenterates and tunicates.

We have thus seen that in certain amphibia and other animals the nervous system has been sufficiently evolved to exert a controlling power over the tissue metabolism. The only other conditions necessary to convert these animals into warm-blooded, or homoiothermic, animals of even temperature, would therefore appear to be an increased heat production of the tissues, coupled with a modification of the external covering of the body so as to diminish the heat loss. Thus the temperature of fish and amphibia is, as a rule, only a fraction of a degree above that of their surroundings. Now the actual carbonic-acid discharge of a

bird, which has a body temperature of over 40° , is only some ten to twenty times greater than that of a frog, of equal weight, at a temperature of 30° , though it is some fifty to a hundred times as great as that of a frog at 15° . With a more efficient protection against heat loss, it therefore follows that the temperature of cold-blooded animals would be several degrees above that of their surroundings. Thus it would need a proportionately much smaller degree of heat-production to keep an animal 5° above the environmental temperature than to keep it 25° . In a few cases it has been shown that in cold-blooded animals the body temperature may be raised considerably above the external temperature. Thus Davy¹ found the temperature of the shark to be 1.3° , and of the bonito (*Thynnus pelamys*) to be no less than 10° above that of the water in which they were kept. Again, the temperature of reptiles may be raised still more considerably. John Hunter² found the temperature of a viper to be raised 5.6° , and Sclater³ found the temperature of a male python on two occasions to be respectively 9.0° and 8.8° , and that of an incubating female no less than 12.7° and 20.0° above that of the air. Forbes made similar observations,⁴ and found the temperature of a male python to be 6.4° , and of a female 9.3° , above the air temperature. Still again, Dutroche⁵ finds that the temperature of the green lizard (*Lacerta viridis*) may be from 4° to 7° above that of the atmosphere. The high temperature which beehives may attain is well known. Thus Newport⁶ found that on one occasion, when the temperature of the air was -7.5° , that of a hive was -1.1° . When the bees were disturbed by tapping the hive, the temperature was raised to 21.1° in fifteen minutes. On another occasion, when the external temperature was 1.4° , that of the hive full of active bees was 38.9° . It is obvious, however, that the bees can

¹ *Researches*, vol. i., p. 189, 1839.

² *Works*, Palmer's edition, vol. iv., p. 131, 1837.

³ *Proc. Zool. Soc.*, 1862, p. 365.

⁴ *Proc. Zool. Soc.*, 1881, p. 960.

⁵ *Ann. des Sciences Nat.*, xiii., p. 20.

⁶ *Phil. Trans.*, 1837, pt. ii., p. 253.

only produce such a considerable elevation of temperature when large numbers of them are confined in a small space. A single bee exposed to a low temperature soon becomes torpid and almost motionless.

We now come to the very interesting observations which have been recently made upon the temperatures of monotremes and marsupials. Upon the duck-billed platypus (*Ornithorhynchus paradoxus*), which is the lowest of the monotremes, and in fact the lowest member of the whole class of mammals, two observations, both on the same individual, have been made by Miklouho-Maclay.¹ As a mean, the temperature was found to be 24.8° , or only 2.6° higher than that of the water in which the animal was kept. It would therefore seem to be more closely related to the cold- than to the warm-blooded animals. The same observer also made several observations² upon another monotreme, *Echidna hystrix*. The mean temperature was 28.0° , that of the air being 20.0° . Semon³ also made several determinations, and found the average rectal temperature to be 32.5° . Sutherland⁴ has recently investigated the subject more fully, and has determined how the temperature of the animal is affected by that of its surroundings. The following are the means of the results obtained by him:—

Number of Observations, each on 3 to 6 individuals.	Average Temperature of Air.	Average Temperature of <i>Echidna</i> .
3	14.1°	25.7°
4	17.4°	28.2°
9	22.1°	30.2°
1	31.2°	32.2°
1	45.0°	36.6°

Here we see that the temperature of these animals is affected very considerably by that of the environment, and that by increasing the temperature from 14.1° to 45.0° , the mean temperature of the animals rose from 25.7° to 36.6° . Even these figures do not express the extreme temperature

¹ *Proc. Linn. Soc. N. S. Wales*, ix., p. 1204.

² *Ibid.*, viii., p. 425.

³ *Arch. f. d. ges. Physiol.*, Bd. 58, S. 229, 1894.

⁴ *Ibid.*

variations of which the animal is capable, as the temperature of one individual on a cold morning was found to be only 22° . Altogether twenty-seven series of observations were made on fourteen different specimens, the mean temperature being 29.4° . This animal in question thus appears to form a true intermediate stage between the cold-blooded animals on the one hand, and the warm-blooded on the other. It is able to keep the temperature of its body considerably above that of the environment, provided this be low, or considerably below it provided this be high; but it is, nevertheless, affected very largely by these external temperature changes, though not to so great an extent as the body temperature of a true cold-blooded animal.

Upon the marsupials, Sutherland made 126 observations, upon sixteen different species. The mean temperature of all of them was 36.0° , or considerably higher than that of the monotremes, but still appreciably lower than that of the true placental mammals. In the following table are given the mean temperatures of the individuals of each genus examined:—

Number of Observations.	Animal.	Temperature.
2	Wombat (<i>Phascolomys</i>) - - - -	34.1°
5	Flying squirrel (<i>Petaurus</i>) - - - -	35.7°
83	Koala (<i>Phascolarctos</i>) - - - -	36.4°
—	„ (excluding females at breeding time)	36.0°
—	<i>Dasyures</i> - - - - -	36.0°
22	Ring-tailed opossum (<i>Phalangista</i>)- -	36.6°

Even among these marsupials we see that the mean temperature varies from 34.1° to 36.6° . The extreme limits of temperature amongst individuals of the same species are even greater. Thus in healthy specimens of the koala, temperatures varying from 34.9° to 38.4° , or by 3.5° , were noticed. Now in healthy placental mammals under normal conditions the limits of temperature rarely exceed one or two degrees. The koala was also influenced by the temperature of its environment to a greater extent than is the case with placentals. Thus as a mean of three observations.

at 9.7° the body temperature was 35.3° , and of six observations at 19.4° , 36.2° .

Finally, on members of the kangaroo family, four observations were made, with the following results:—

<i>Pterogale xanthopus</i> ,	35.9°
<i>Macropus giganteus</i> ,	36.6°
<i>Dendrogalus grayi</i> ,	37.0°
<i>Halmaturus bennettii</i> ,	37.1°

In the last two of these animals we see that the temperature of placental mammals was reached. For the sake of comparison, a table is appended showing the mean rectal temperature of some of the better known of these mammals.¹

Man	-	-	-	-	37.0°	Dog	-	-	-	-	38.6°
Horse	-	-	-	-	37.8°	Cat	-	-	-	-	38.7°
Monkey	-	-	-	-	38.4°	Rabbit	-	-	-	-	38.9°
Cow	-	-	-	-	38.6°	Sheep	-	-	-	-	40.2°

These we see to vary from 37.0° for man to 40.2° for the sheep.

We have now arrived at the true placental mammals, and it remains for us to inquire how far these resemble and differ from cold-blooded animals in the reaction of their temperature and respiratory activity to changes in the temperature of their surroundings. It is generally stated that warm-blooded animals differ from the cold-blooded in exhibiting an increased respiratory activity at low temperatures and a diminished one at high temperatures, and also that the nervous system is able to regulate the heat production and heat loss so efficiently that the body temperature is practically uninfluenced by that of its surroundings. We shall see that under certain conditions both of these relations are departed from, and that thereby a connection with the cold-blooded animals is established.

As is but natural, these questions of respiratory activity

¹ These data are taken from the table given on p. 790 of Schäfer's *Text-book of Physiology*, vol. i.

and body temperature have been studied most fully in the case of man, and hence we shall refer to the results so obtained at some little length. Thus Liebermeister¹ determined the effect on the respiration of baths of various temperatures, and found that the carbonic acid output increased regularly with decrease of temperature of the water. Voit² exposed his subjects of experiment in a respiration apparatus to air temperatures varying from 4·4° to 30°. In order to diminish the experimental errors as much as possible, no food was taken by the subjects for sixteen hours previously, and every experiment extended over six hours. The carbonic acid output was found to diminish regularly from 210·7 gm. per six hours at 4·4°, to 155·1 gm. at 14·3°. From this point it slowly but rather irregularly rose again, till at 30° the output was 170·6 gm. Thus the respiratory activity did not vary regularly with the temperature, but reached a minimum value at an intermediate temperature. A similar reaction to temperature has been observed in other mammals. Thus Page³ determined the carbonic acid discharge of a dog at temperatures varying from 15° to 35°, and found that the metabolism reached its minimum at 25°. Rubner⁴, working with guineapigs, obtained the following results :—

External Temperature.	Temperature of Animal.	Carbonic Acid output per kils. per hour.
0°	37·0°	2·91 gm.
11·0°	37·2°	2·15
20·8°	37·4°	1·77
25·7°	37·0°	1·54
30·3°	37·7°	1·32
34·9°	38·2°	1·27
40·0°	39·5°	1·45

Here we see that the external temperature varied from 0° to 40°, and that the metabolism reached its minimum at

¹ *Deutsch. Arch. f. kl. Med.*, Bd. x., S. 75, 1872.

² *Ztschr. f. Biol.*, Bd. xiv., S. 79, 1878.

³ *J. Physiol.*, ii., p. 228.

⁴ *Biologische Gesetze*. Marburg, 1887.

34·9°. This table is introduced chiefly, however, in order to illustrate another point, *viz.*, the effect of the environmental temperature on that of the animal under experiment. We see that with a single exception the body temperature rose regularly, with the rise of external temperature, from 37·0° to 39·5°. This rise is almost as considerable as was observed by Sutherland in the Koala, though not nearly so great as in *Echidna*, and it goes to prove that in the guineapig, at least, the power of regulation of the body temperature is by no means perfect.

To return to the experiments on man, Loewy¹ made numerous observations in which the subject of experiment lay at rest on a sofa, and had his respiratory exchange determined when he was in a clothed and unclothed condition. In other experiments warm and cold baths were used. The chief point of these experiments lay in the fact that especial attention was paid to the presence or absence of shivering and other movements of the subject when exposed to cold. It was found that the oxygen intake remained uninfluenced by the exposure to cold in twenty cases, and was diminished in nine of them. It was increased in twenty-six cases, but in thirteen of these shivering and muscular movements were observed, and it was probable that in the other cases there were also some such movements, though not sufficient to be observed. It would therefore seem that on exposure to low temperatures increased movement is the chief if not the only means the nervous system possesses of increasing the heat production of the body. This conclusion has been confirmed by the numerous experiments recently made by Johansson,² as also by the fact that in the various experiments which have been made on dogs, guineapigs, rabbits and mice it was always found that the animals were more active and showed increased movements on exposure to lower temperatures. In Johansson's observations, the subject of experiment was seated or lay in bed for a certain time, and the carbonic acid output was determined. All the clothing was then removed, and the carbonic acid out-

¹ *Pflüger's Archiv.*, Bd. xlvi., S. 189, 1890.

² *Skandinavische Archiv. f. Physiol.*, Bd. vii., S. 123, 1896.

put again determined over several intervals of a quarter or half an hour. The subject then resumed his clothing, or lay covered up in bed again, the carbonic acid being further determined. Johansson found, as had Loewy, that the respiratory activity was considerably increased if shivering and other movements were induced by the cold, but concluded that in those cases in which these movements were kept in abeyance by an effort of will, the carbonic acid output was practically uninfluenced by the temperature of the surroundings. In the following table are shown the results he obtained in support of this contention. The temperatures given are those of the air in the respiration chamber.

Temperature of Air.	Per cent. change of CO ₂ output during cold period.	Fall of rectal temperature during cold period.
13·7°	— 8·9 per cent.	—
14·6°	+ 12·2 „	— ·8°
15·3°	+ 13·3 „	— ·9°
16·7°	— 5·1 „	—
18·8°	+ 1·3 „	— 1·1°
19·8°	+ ·8 „	— ·6°
20·6°	+ 1·6 „	— ·68°
20·7°	+ 13·2 „	— ·2°
21·5°	+ 5·5 „	— ·5°

From these values it will be seen that on an average only 3·8 per cent. more carbonic acid was discharged when the subject of experiment was unclothed, than when he was clothed. Also it would appear that the respiratory activity was no greater when the temperature of the air in the respiration chamber was low, than when it was 7·8° higher. It is noticeable, however, that the air temperature had an appreciable influence on the actual body temperature of the subject. Thus with lower air temperatures, the fall of body temperature was from ·8° to 1·1°, and with higher, only ·2° to ·68°.

As a whole, therefore, these experiments on man and the lower animals prove that the power of regulating the body temperature is not by any means perfect, and there is no very conclusive evidence of the existence of a special

mechanism which can bring about an increased metabolism and heat production of the muscles and other tissues of the body, apart from actual muscular movements. Thus, if these are controlled by an effort of the will, as in these experiments on man, the body temperature seems to fall appreciably ; whilst in the guineapig, where, of course, no such voluntary control is exercised, we have seen that the nervous system is unable to keep the body temperature constant. In the long run, however, it appears that the nervous heat regulating mechanism can perform its function more efficiently, and that it is only when the animal is exposed to somewhat rapid variations of external temperature that it is much at fault. Thus, as the result of numerous observations by Davy,¹ Rattray,² Crombie³ and others, it has been found that the body temperature of man in the tropics is only a fraction of a degree above that observed in temperate zones. In arctic regions also, it suffers no diminution. Under these circumstances, the body must obviously be losing much less or much more heat than in temperate zones, and yet the nervous system is able to counterbalance the varying loss.

We thus see that the warm-blooded animal is constituted similarly to the cold-blooded one in some respects. Increase of temperature does tend to send up the body temperature, and to bring about an increased metabolism, but it is only under somewhat exceptional circumstances that the counterbalancing nervous influence is unable to more or less effectually cope with this ever present tendency. Under abnormal conditions the mechanism may be totally at fault. Thus numerous instances are recorded of drunkards who have been left exposed on cold nights, and in whom the body temperature has fallen to as low as 24.0° , 24.7° and 25.0° , but with subsequent gradual recovery to the normal temperature.⁴ In cases of fever again, the heat regulating mechanism is also found wanting ; in some cases, as of

¹ *Phil. Trans.*, 1850, p. 437.

² *Proc. Roy. Soc.*, vol. xviii., p. 526, 1870.

³ *Indian Ann. Med. Soc.*, vol. xvi., p. 550, 1873.

⁴ *Vide Schäfer's Text-Book of Physiology*, vol. i., p. 821.

acute rheumatism and scarlet fever, to such an extent that death would appear to ensue directly from the great rise of temperature causing coagulation of some of the proteids of the tissues. Still again, in the case of certain hibernating mammals, we know that the nervous system, though still in perfect working order, permits the temperature of the body to fall as low as 2° , and to be but a fraction of a degree above that of the surroundings. Yet in these cases the heat regulating mechanism is only temporarily in abeyance. If irritated, and caused to awake from their torpor, these animals are able in a very short space of time to raise their body temperatures through a considerable temperature interval. Thus Pembrey and Hale White¹ record a case in which a dormouse was observed to raise its temperature from 13.5° to 35.75° in an hour's time. The existence of these hibernating animals proves that there is no absolute barrier between the warm- and cold-blooded animals. Non-hibernating warm-blooded animals may, indeed, for the time being, be practically converted into cold-blooded ones by the administration of curare, or by section of the spinal cord in the cervical region. In animals so treated, the carbonic acid output and body temperature rapidly fall, and only rise again on exposure to artificial warmth.

Just as there has been a physiological evolution in the direction of increased body temperature as we pass from the lower orders of mammals to the higher, so we find a somewhat similar condition of affairs amongst the various orders of birds. Thus of the Ratitæ, the lowest of the orders, the cloacal temperature of the ostrich was found by Hobday,² as a mean of observations on five different specimens, to be only 37.3° , whilst the temperature of the emu was found by Le Souef³ to be 39.5° . Of the orders of Anseres, Columbæ and Galli the following data have been obtained.⁴

¹ *J. Physiol.*, vol. xix., p. 477.

² *Journ. Comp. Path. and Therap.*, vol. ix., p. 286, 1896.

³ *Vide* Sutherland's paper, *ibid.*

⁴ *Vide* Schäfer's *Text-book of Physiology*, vol. i., p. 791.

Bird.	Average Cloacal Temperature.	Bird.	Average Cloacal Temperature.
Pigeon - -	40·9°	Duck - -	42·1°
Goose - -	41·7°	" - -	43·6°
Fowl (common) -	41·6°	Pheasant - -	42·6°
" - -	42·8°	Turkey - -	42·8°
		Guinea-fowl -	43·3°

With these birds the average cloacal temperature is only about 42°. In the Passeres, however, which form the highest order of the class Aves, the temperature is, as a rule, a degree or two higher.

Bird.	Average Cloacal Temperature.	Bird.	Average Cloacal Temperature.
Sparrow - -	42·1°	Fieldfare - -	43·7°
Thrush - -	42·8°	Swift - -	44·0°
Yellowhammer -	43·2°	Great titmouse -	44·0°
Redwing - -	43·3°		

Why there should be this gradual rise of temperature accompanying the increasing complexity of morphological structure is by no means clear. That the two conditions do not necessarily go hand in hand is shown by the above quoted data on the temperatures of certain of the higher mammals. From these it may be seen that the temperature of man is lower than that of any of the other mammals mentioned, and no less than 3·2° lower than that of the sheep. A high body temperature is certainly an advantage in one way. Thus the more it is raised above that of the environment, the easier is it for the nervous system to keep it approximately constant. On the other hand it obviously entails a higher metabolism and increased consumption of food, a weighty drawback in view of the ever present struggle for existence. Probably the temperature is to some extent bound up with the size of the animal. Thus the smaller this is the more would variations in the external temperature tend to produce variations in body temperature, and the more sensitive would the heat regulating mechanism need to be in order to keep the body temperature constant. Thus if the above data on birds be re-examined, it will be

seen that the members of the lowest order, *viz.*, the ostrich and emu, are also the largest individuals upon which observations were made, whilst those of the highest order, the Passeres, are the smallest. This apparent interdependence of size and body temperature may be a coincidence, but it is nevertheless sufficiently striking to be worthy of mention.

Just as in morphology we know that, according to the law of von Baer, the developmental history of the individual to a certain extent recapitulates the developmental history of the race, so also does a somewhat similar condition of affairs exhibit itself in regard to the physiological question under discussion. The embryonic warm-blooded animal, which has descended from a cold-blooded ancestor, is itself cold-blooded. Thus the physiological evolution of the chick, in relation to its reaction to external temperature, has been recently worked out very carefully by Pembrey, Gordon and Warren.¹ On determining the respiratory activity of eggs at various stages of incubation, these observers found that the developing chick, during the greater part of its period of incubation, responded to changes of external temperature like a cold-blooded animal. About the twentieth to twenty-first day, however, an intermediate stage was noticed, in which there was no marked response in either direction. When the chick was hatched, this neutral stage was succeeded by a stage in which it reacted as a warm-blooded animal. On lowering the external temperature, it now showed more active muscular movements, and gave out an increased amount of carbonic acid. Guinea-pigs, shortly after birth, were found by Pembrey² to react in a similar way, but mice, rats and pigeons, which are born blind, naked and helpless, react in the same way as cold-blooded animals. On exposure to cold their carbonic acid output is considerably decreased, and their body temperature also falls. The power of heat regulation was found, however, to be well developed in mice by the tenth day after birth, and in pigeons by the fifteenth to sixteenth day. These animals had then attained their full and true status as warm-blooded animals.

H. M. VERNON.

¹ *J. Physiol.*, xvii., p. 331.

² *Ibid.*, xviii., p. 363.



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PAPER AND PAPER STANDARDS.¹

THE editor of this periodical has requisitioned a contribution on the subject of "Paper". The author of "these presents" is perhaps directly responsible for a slight disturbance of official serenity in this matter. We all commit ourselves *to* paper, and for better for worse the *litera scripta manet*, or should do. The suggestion that this dictum may be falsified by the unexpected dissolution of the paper which carries our precious cargo of records on the ocean of time is sufficient ground for at least momentary pause to our official publishing bodies.

The suggestion is, we admit, a little *ad captandum*. But it is pardonable as an opening to the consideration of the much broader subject of Paper Standards.

Paper, or rather papers, are products of industrial evolution. The selection has been the "natural" one, and the result is a compromise; in every direction compromise. If the average Englishman (himself also a compromise) were asked to name his ideal in paper, he would, of course, for mixed but none the less obvious reasons, name the Bank of England Note. It will be considered an impertinence on our part to express a doubt as to whether this prototype of conventional excellence in paper is not capable of important improvements; from our point of view, however, there

¹ An informal discussion of matters dealt with in Report of Soc. of Arts Committee on "Deterioration of Paper". *Journ. Soc. Arts*, 1898, 46, 597. See also *ibid.*, 1897, 45, 690-696.

is not even a doubt. Of course it is improvable, and for reasons which will presently appear.

Our Art papers—what of them? All of course excellent, for are they not the product of half-unconscious selections accumulated through generations? Tested, however, by the standards of science, they are found wanting.

But—we are still following the *ad captandum* method and perhaps gratuitously—we do not know that it is necessary to base an appeal for the extension of scientific method to a hitherto unappropriated field by a demonstration of defects in that field, and corresponding remedies resulting from their more exact scientific study. To the popular judgment it is a very concrete proof of potential progress, and may be interjected as the necessary stimulant to positive thinking. Readers of SCIENCE PROGRESS are accustomed to another discipline. Whether as actual investigators or as appreciative critics they recognise an element of passion or sentiment in the mere extension of the intellectual horizon, in penetrating into the inner workings of things, which justifies effort and makes work self-sufficient and independent of outside stimulus. So let it be with them in all that appertains to paper, regarded as an aggregate of well-defined chemical individuals and structural units—in other words, as an eminently fit subject for scientific treatment.

Of course a great many of us have taken the commodity into use without inquiring as to what it is. Life in fact is not long enough for more than superficial general knowledge. Outside that small field in which we may enjoy the specialist's privilege of feeling "at home," we must accept the services of the guide-book or the "personal conductor" and be content with knowledge more or less second-hand. Our justification is in the well-worn truism: *Ars longa vita brevis*; but always qualified by the schoolboy's translation "Life is short but the ears of the ass are long," providing a salutary reminder that a "little knowledge," of the second-hand order, may be a "dangerous thing". An apt illustration comes to hand from actual and not very remote experience. The author was approached by an enthusias-

tical empiric, supported by an amiable, *i.e.*, long-suffering, capitalist, who had undertaken a scheme of making paper from "spent" hops. Of course it is known to the enlightened that paper can be made from anything from thistle down to deal boards, or should the vegetable world fail us, from asbestos to china clay. The initial advantage of a raw material costing nothing had appealed to the capitalist, and his empiric "will-o'-the-wisp" was justified by the above luminous generalisation rising from the morass of technical stagnation. But the point of our story is in a later episode. Our pioneer friend had been advised to boil his spent hops with *milk of lime*. An order had been sent to one of our wholesale druggists (sometimes mis-named "chemists") for a supply of the material, and when we came on the scene we found that to this order *Calcium lactate* had been supplied and was being freely used in the boiling of the spent hops.

For the pathos and comedy of spent hopes we doubt whether the chemist's experience can be equalled!

We have not forgotten that we are writing about a specific subject, neither have we forgotten that readers of SCIENCE PROGRESS are not of the order to be impressed by text-book disquisitions on scientific matters. The technical history of paper and the chemistry of the raw materials of which it is composed are oft-told stories. What we may point out without risking an accusation of tedious iteration, is that the writing and printing papers of our date are extremely "select" as regards raw materials. Of the hundred and forty and four thousand possible sources of supply, we have narrowed down to some half-dozen of the vegetable fibrous substances. Thus of the dicotyledons, one seed hair (cotton) and two bast fibres (flax and hemp); of the sub-class gymnosperms the wood-cells (fibres) of the stems of conifers; of the monocotyledons the stem tissues of the cereal straws and of esparto.

These materials are employed not in their raw condition. They are chemically treated for the isolation of cellulose. The treatments are designed to remove their more reactive or non-cellulose groups and to leave a chemically inert or non-reactive residue. Such is the com-

plex which we designate cellulose. Judged by the chemical criterion the paper-maker's celluloses divide themselves into three well marked groups which we may designate by their typical representatives as the Cotton, Wood and Esparto groups; and these groups correspond with the classification of the botanist.

This has only to be pointed out in specific terms to those accustomed to the positive discipline of natural science and they will start at once to inquire whether the sheet they are now handling is of the rag, wood or esparto class, knowing perfectly well that important consequences must follow from fundamental differences of composition, with the added significance of differences of physiological function. Some of these consequences are easily brought to demonstration. Take a "Swedish" filter paper as a normal standard, serving as basis of comparison, and select a paper of each of the above classes, as used for writing or printing purposes. Such papers it must be premised contain a proportion of "sizing" constituents; that is, they consist of the respective celluloses with "organic" colloids, *e.g.*, gelatine or starch, and fatty or resinous bodies precipitated into the sheet from alkaline solutions by means of sulphate of alumina. These constituents, jointly and severally, contribute a certain water or ink-resisting quality, which we may define in precise terms as so controlling or limiting the absorption of aqueous solutions that it takes place in the vertical (downward) direction only. It is necessary to mention these qualifying or auxiliary constituents as they contribute directly to the effects we are about to describe. We have asked the reader to "select" papers of the typical classes; but on the assumption that he may probably be doing this for the first time some directions are required. There we meet a difficulty. It might occur to the layman that he has only to go to "the Shop," there to ask and to get. But stationers are notoriously stationary. Their classification of papers is strictly limited to such side issues as price, "get up," colour and so forth. The shopman's vision generally is limited by the horizon of values, that simple calculus of differences by which the

“steady three to two against the purchaser” is maintained ; he seldom condescends to things. The best way out of this difficulty is to adopt a plan somewhat as follows :—

For a paper of Class I. your lawyer's letters will supply specimens : the lawyer knows the value of a letter both to himself and client ! The profession knows no laws so binding as those of precedent and convention, and therefore it jealously conserves such papers as it has known through the ages, such also as it knows will preserve our “acts and deeds” for all time.

For Class II. look up letters from friends in Germany. The chances are several to one that these will be “cellulose” papers. Cellulose we must explain in German technology means “Wood Cellulose”. The limitation is the basis of a small quarrel which we have with our cousins ; we are getting the best of it and shall succeed in “depolarising” the term and restoring to it, even in Germany, its general significance.

For Class III. letters from members of the “lower middle class,” or publications, we regret to say, of many of our learned Societies will supply specimens.

Having by these indirect means obtained authentic specimens they may be put through the following simple tests : (1) Place the specimens each in a stoppered bottle containing a few cc. of water. Set aside in a “warm corner” and after ten or fourteen days note what has happened. Papers of Class I. will have proved themselves an excellent nidus for micro-organisms of all types, colonies of these will have established themselves after their manner and gorgeous effects in yellow, crimson, and blue will reward the observer. The filter paper (which by the way must not be allowed to come in actual contact with the water) will not show any such effects. They are obviously due to the nitrogenous colloid, the gelatine, used in sizing the paper, for as regards the cellulose fibres of which they are composed the two papers may be considered as identical. Probably also the papers of Classes II. and III. will not have grown any organisms. In other words, the pure celluloses are not susceptible to the direct attack of organisms. But given a supply of the

necessary nitrogenous and saline nutrients they yield more or less readily in the inverse order of our classification. They yield, by undergoing hydrolysis to soluble products allied to the starch-sugar series, capable of assimilation by living organisms. The celluloses of the cereals and of esparto are very readily so attacked, and for this reason the tissue constituents of the straws are considerably digested in their passage through the digestive tract of the herbivora. Precisely for this reason the celluloses of straw or esparto rank very much below the normal or typical cotton cellulose, as paper-making materials. The wood celluloses are intermediate.

(2) The selected papers may be exposed to the heat of a water-oven and the effects noted from time to time. Such effects result in short time, as over lengthened periods of storage under ordinary conditions. Papers of Class III. will discolour very rapidly and at the same time become brittle. In regard to the action of dry heat again the papers will be found to range themselves according to our classification, as they have been found to do in reference to permanence under ordinary conditions. The changes determined by heating are not only chemical but structural: those physical properties upon which the qualities of the fibre-agglomerate depend, are in considerable measure properties of the fibre-substance itself, and as the constitution of the cellulose is changed under the joint influence of the temperature and the saline constituents of the paper, there follow changes in breaking strain and elasticity.

As already explained it is not the purpose of this article to enter into a minute discussion of the technics of paper, nor do we think that in these days science needs to "labour" a case for the consideration of the lay-public nor should be compelled to come cap in hand to the manufacturer and beg to be allowed to show him how to increase his profits. That, by the way, is the one question which the scientific man does not allow to be a question at all. If the manufacturer wishes to accept the challenge of science, it must be somewhat in these terms:—

(1) As regards quantity: Can you account for every

pound of raw material taken into work? and (2) as regards quality (of manufactured product): Is this regulated by normal standards combined with systematic records? The above being the scientific conditions of routine production.

And then (3) as regards improvements and discoveries: Are you organised not only to take note of all forward movements from outside, but to ensure a proportionate probability of personally securing a lead in the large technical advances which are being made and to be made? The manufacturer, in other words, must seek first the kingdom of technology, and all the details of large profits will be duly added.

Our friend, the unconverted paper-maker, smiles. We know that smile. It is the smile of the man who would sleep a little longer.

And what of the consumer of paper? His name is legion and it is of little avail to work for a universal public opinion upon such a subject. We can only make our appeal to those who can mould or constitute such a stress of opinion or judgment as shall gradually bring about a reform which in other countries can be determined from the head-quarters of a parental government. It will be enough to convey to scientific men that there is a large field of useful and extremely interesting work to be done in bringing science to bear upon the vastly important industry of paper.

As for special problems, they are innumerable and the solutions are at hand.

To return to our "ideal" Bank of England note. We can produce that gelatine-free, *i.e.*, nitrogen-free; similarly our water-colour or drawing papers, our photographic papers, can be produced with at least this safeguard against mould and bacterial growth; our "Art papers" can be produced with something less than 30-40 per cent. of mineral matter or "filling," and we can make papers adapted to a hundred and one of the uses to which it is not as yet applied.

What a blessing that the absurd phrase *fin de siècle* has already begun to pass into the limbo of antiquities! Probably also in the next century we shall have no

“Baxterisms” to feed the catastrophic mind, and we shall start on a fair career of further industrial evolution. We convey the hint to those who are minded to play their part in this scheme that the subject of cellulose is worth attention. It can never be said of the cellulose industries that they are played out or ever likely to be. This by the way.

But we can commend our subject on other than grounds of personal enterprise, that is, on the basis of industrial ethics or morals. One of our thinkers wrote it down as a crime that a child should grow up uneducated. What are we to say to such ignorance of “things” as leads people to buy “linen” handkerchiefs for 2s. 6d. a dozen, to pay 4d. per lb. for soda ash—bought as “Diddler’s Detergent”; ten shillings a lb. for permanganate of potash because sold as “I. Prices’s Chameleozone,” and to buy china clay at the rate of 1s. per five quires because it is called “paper”?

Is this the ignorance of the blissful order which ’twere folly to disturb? Is it in the code of natural science “criminal” as implying divorce from the realities of the world?

Whatever it may mean in the popular code, the scientific mind cannot see these things and pass by on the other side. It may be picturesque and may be amusing. But we need not adopt a close season for eccentricities and perversities for fear that intellectual sport will die out for want of game. There is a fresh supply of the more ignorant to reach maturity every day, and these are no doubt providentially supplied for the less ignorant to “operate” upon; also there is evidently a sublime law of the ridiculous. Therefore we may go forward and wage the battle of the paper standards. It cannot spoil sport nor do any honest man any harm and it will do science the good of extending its dominion to a most “legitimate” and desirable province.

C. F. CROSS.

ON SELECTION IN MAN.

MY last article dealt with processes of human selection dependent on or connected with differences of colour: in the present one I propose briefly to discuss similar processes connected with differences of headform, and especially with dolichokephaly and brachykephaly. Colour yields the most conspicuous and in some respects, perhaps, even the most important of the signs of difference between varieties of mankind; but diversities of headform are generally believed to be more permanent, though this has never been absolutely proved.

The starting-point for investigations of this subject may be said to have been given by Durand de Gros; though, when he affirmed that in the Rouergue the heads of town-folk were longer than those of peasants, he did not at first divine the cause of this phenomenon, but ascribed it to the operation of "media," of some subtle influence of environment. At a later period, indeed, he began to see the relevance of the principle of selection; but it was De Lapouge who first saw its pre-eminent importance, and who applied it generally, not only to urban and rural populations, but to the different strata of society and periods of history. Working in the south of France, with Montpellier for his centre, he showed that the modern population of that town, formerly much longer-headed than the peasantry of the Herault, was in the way of losing this characteristic; also that the Languedocian nobility of the sixteenth to the eighteenth century had been dolichokephalic in the midst of a short-headed peasantry; and that a similar difference prevailed in the eighteenth century between the upper and the lower class of citizens in Montpellier. Thus—

	Montpelier at present.	Montp. 18th Century.		Peasants at present.	Seigneurs 17th and 18th Century.
		Upper Class.	Lower Class.		
Mean kephalic index (relative head-breadth) -	81	75	78	82	76½

Now the Herault is a comparatively rich and level region, with a population of very mixed race, yielding to the callipers mesokephalic and sub-brachykephalic indices, while the nobility, Gothic or Galatic in its original strain, may be taken to be mainly composed of the descendants of those who have raised themselves to wealth by ability and energy. But on its northern border is the Aveyron, a poor, rugged, elevated district, with a decidedly broad-headed population of the Alpine race or type. In accordance with the well-known rule in such cases, there is, and has long been, a constant stream of migration from the Aveyron to the Herault, to which the analogous districts of the Tarn and the Lozère also contribute.¹ A necessary result is a gradual and slow increase in the kephalic index of a city such as Montpellier, from the relative growth of the brachykephalic element. And this process would have been much more rapid but for the operation of a law, the discovery of which we owe to De Lapouge and Ammon, *viz.*, “the greater readiness to migrate of the dolichokephalic element in a mixed population,” whence it comes to pass that the Aveyronese emigrants are not so broad-headed as the population they leave behind them, in the ratio of 84 to $85\frac{1}{2}$.

De Lapouge follows Gobineau in regarding the tall, blond, long-headed breed of Northern Europe (the Aryan race as some would call it, though I regard the term as inaccurate and objectionable) as the salt of the earth, the stirring, active, ambitious, independent, courageous, locomotive element of mankind; and it appears to be this character which is connoted by that projection of the occiput, which is more common in the inhabitants of these islands than in those of almost any other country.² “The

¹ The Aude, a comparatively long-headed district at the foot of the Pyrenees, also furnishes a contingent, but not large enough to counter-balance the Aveyronese.

² An English naturalist made an unexpected visit to Prof. Fitzinger, the eminent Viennese anthropologist. “Welcome!” said Fitzinger, “I don’t know who you are; but I can see you are a Briton by the back of your head.”

dolichokephal has great ambitions," says De Lapouge,¹ "and strives without ceasing to gratify them. He knows better how to gain than to keep riches. He dares everything ; and his audacity ensures success. He fights for fighting's sake, but not without some idea of profit behind. Every land is his : the whole globe is his country. His intelligence varies from stupidity to genius. He is logical when it suits him, and not to be put off with words. Progress is his greatest need. In religion he is Protestant ; from the State he only demands freedom of action, and seeks rather to elevate himself than to depress others."

The other type with which we have most to do is the brachykephalic Alpine, sometimes, but perhaps inaccurately, called Kelto-slav.² In this the stature is short, the head is round or trapezoidal, flattened behind, the coloration brown or dark. "The brachykephal is frugal, laborious, or at least economical, remarkably prudent, and though not cowardly, yet not warlike. His intelligence is usually mediocre, and he works out patiently his limited ideals. Though suspicious, he is easily taken in with words. He is the slave of tradition, and of what is called common sense. He distrusts progress, and adores uniformity. In religion he is willingly Catholic : in politics, he has but one hope, the protection of the State, and but one tendency, to level down, caring little to elevate himself. He sees clearly his own immediate interest and that of his family and neighbours, but that of his country is too remote for him."

In the mixed breeds which largely prevail in France, "the egotism is reinforced by the energetic individualism of the dolichokephal, and the sentiment of family and of race is weakened : hence come the vices of which our bourgeois are accused, winding up with elimination by 'self-restraint'".

We all know that while dolichokephaly, even of extreme degree, prevails among the relics of the palæolithic

¹ I have somewhat abridged this very complete and coherent portrait, as well as that of *Homo Alpinus*, which follows.

² The brachykephaly of the earliest known Slavs is very doubtful, to say the most of it.

and even of the neolithic period, brachykephaly is extremely uncommon. Nevertheless it does occur ; but still more seldom does it appear in an extreme form. Most of us are of opinion that the ancestors of the modern " Alpine " brachykephals were neighbours of the Iranian Galchas, and pushed westwards into Europe during the neolithic period, possibly bringing with them some twigs of the Mongoloid stem, and incorporating any western brachykephals whom they met with. Also we think that the paucity of remains of this race in the later prehistoric or earlier historical periods may have been due to their position as serfs to the higher race, which excluded them from elaborate modes of sepulture. But Lapouge takes another view, which I believe to be original. He thinks we have no grounds for believing in such a westerly migration : he would derive the modern from the prehistoric western shortheads, and explain their rarity in tombs simply by their relative paucity in the populations. That paucity, he thinks, may have grown into multitude by the action of such processes of selection and elimination as he and Ammon have found and demonstrated as operating at the present day. In a word, he rejects, like Lyell, hypotheses of catastrophe.

What these processes are may perhaps be best seen in the work of Ammon. The material of this was gotten by observation of the conscripts and scholars of the Grand Duchy of Baden, especially those of the cities of Karlsruhe, Mannheim and Freiburg-in-Brisgau ; and when gotten it was subjected to analysis of the most masterly, minute and multifarious character. For some of his purposes and conclusions this material was ample ; for others perhaps scarcely so. It may be necessary here to point out that for the determination of a question of head-breadth a much smaller number of cases, or of individuals, is sufficient, than for that of a question of colour or complexion. In the former case, as a rule, most of the knowledge sought can be acquired by simply dividing the material into the two classes of long and short heads. In the latter little can be learned, generally speaking, without dividing the material into a large number of classes. Thus Virchow, in his

inquiry into the colours of the German schoolchildren, made use of twenty-one categories : I, myself, always use at least fifteen. Unless, therefore, the aggregate number of specimens examined is very large, the numbers in the least populous categories come to be so small as to depend very much on chance.

To explain Ammon's methods it is almost necessary to reproduce one or two of his tables :—

FREIBURG CONSCRIPTS.

Origin of Subjects.	Kephalic Index.	Mean Head-length.	Mean Head-breadth.
Mean of surrounding district -	83·6	182	153
Immigrants, born in the Duchy -	83	184	154
Half-citizens (father country-born)	82	185	152
Citizens (father city-born) - -	80·8	187	151

Here the man who is attracted to the city has on the average a head longer both actually and relatively to its breadth than the peasant whom he leaves behind him. He is the product of a selective process. But an analogous process would seem to go on after his arrival. The dolichocephal is not only attracted by the city ; he is in some way more suited to a city life. The broader-headed among his immigrant comrades seem either to return unsuccessful to the country or to fail in rearing progeny in the city.

Though the competitive struggle in these South German cities may be in some respects more severe than in the rural districts, it would seem that the whole urban population is better nourished than the peasantry. Ammon at least always takes this for granted in his arguments ; and it is confirmed by the fact that the city-born youth, at the age of conscription, are taller than the country-born, though, as the former are clearly much more forward in the development of the physical signs of manhood, it is almost certain that growth continues in the latter to a later period. Doubtless here also the influence of some kinds of selection enters into the problem.

These conditions are not identical with those which

obtain in our own country. With us it is, as a rule, the lower-class youth of the cities who are worst fed, and their stature is below that of the same class in the rural districts.

The following table, for Karlsruhe, confirms almost absolutely the one for Freiburg.

KARLSRUHE CONSCRIPTS.

	Kephalic Index.	Mean Head-length.	Mean Head-breadth.
Mean of surrounding district -	83	184	154
Immigrants, born in the Duchy -	83.1	184	154
„ born elsewhere -	82.5	186	154
Half-citizens (father country-born)	81.5	186	153
Citizens (father city-born) - -	81.4	186	153
Do, native (father from another town) - - - - -	80.2	184	149
Both subject and father from an- other town - - - - -	81.8	185	152
Born in a town (father countryman)	80.3	184	151
Born in country (father in town) -	82.8	184	153

These facts may be put in another form, in which they are perhaps still more striking.

KARLSRUHE.

	Average of Baden.	Immigrants.	Half-cits.	Citizens.
Per cent. of indices below 80 (dolichoid)	12.2	14.9	25.9	33.3
Per cent. of indices of 85 and above - -	38.2	33.3	18.4	12.4

We are now prepared to receive as at least highly probable the “law of Ammon”. “That in regions where the brachykephalic type prevails it tends to become localised in the rural districts, while the dolichoid type becomes concentrated in the cities.” But if it need confirmation, this may be derived in abundance from the observations of Collignon, but more especially from those of De Laponge. Thus in a series of 108 immigrants into the Herault, the latter found an average breadth-index of 82.34, whereas the average calculated from the known indices of the several departments of origin would have been 83.40. Again, in four little towns of the Herault the urbans (issue of parents

both born in the towns) gave indices, Clermont of 79·5, Lodève of 79·7, Limel and Marsillarges of 82·1 and 81·3, while the corresponding rustics gave 84·4, 82·3, and 83·3. And in quite another part of France, De Lapouge found an index of 82·8 in 67 pure urbans of Rennes, but one of 84·7 in 100 country-folk of the circumjacent canton. In Bordeaux and eight other cities of the south-west Collignon found that the average difference of index between the town and the country was about two units, at Bordeaux itself 2·3. Lower Austria, again, is a moderately brachycephalic province, with an index of 82·2 and 25 per cent. of dolichokephals: Vienna yields 81·2 and 37 per cent., its suburbs 81·5 and 32, and country villages 83·8 and 10. Here we have, I think, a pretty distinct case of the selection of a race-type; for many years ago I formed the opinion that the native Viennese were much more Germanic than the surrounding country-folk, among whom features possibly dating from the Avar occupation frequently cropped up. In connection with this "law of Ammon" several other "laws" have been laid down, chiefly by De Lapouge. One of these is the "law of altitudes" according to which, "in regions inhabited jointly by *Homo Europæus* (the dolichokephal) and *Homo Alpinus*, the former is concentrated in the lower levels". Facts illustrative of this law are so conspicuous that Johannes Rauke some years ago seemed inclined to think that an elevated habitat did in some way favour the production of a short-headed race. That is, of course, highly improbable; and it may be noted that very high averages of brachycephaly are found in some very level and lowlying regions, such as Poland, and Kostroma in Russia. Moreover, the rule does not hold good in southern Italy, where the two races in contact are the Alpine and (not the European but) the Mediterranean dolichokephal. Still, it is of very wide application and by no means confined to Europe. The causes are partly political, partly selective. A conquering tribe seizes first the territories least easy to defend, and retains for itself the most fertile lands—these are usually identical, and lie in the plains rather than in the hills; moreover, the conquering

racés of Europe have almost all belonged to the dolicho-blond type.

Law of Distribution of Wealth.—In countries inhabited jointly by *Homo Europæus* and *Homo Alpinus*, the former element possesses more than its proportionate share of wealth. This law is connected with the last through an intermediate one, *viz.*, that great cities are generally found in dolichokephalic or comparatively dolichokephalic regions, *i.e.*, in the plains; and on the other hand is related to the law of Ammon, because, as we have seen, the dolichoid element tends to concentrate itself in the cities. The test of this rule is tax-paying capacity. De Laponge has compared the thirteen most dolichoid departments of France with the thirty most brachykephalic. The latter are the more extensive and thinly peopled: each group contains about 10,000,000 inhabitants. I have abridged and simplified his table.

			Dolichoid Group.	Brachy Group.
Taxes on Land	-	-	proportion to 100	168
„ Personal Property	-	„	100	50
„ Doors and Windows	-	„	100	50
„ Transfer of Property	-	„	100	56
„ Bequests and Inheritances		„	100	48
„ Leases and Mortgages		„	100	40
„ Stamps	-	-	„	100
„ Beverages	-	-	„	100
„ Tobacco	-	-	„	100
„ Bicycles	-	-	„	100
Receipts of Departments	-	-	„	100
„ Communes	-	-	„	100
Octroi	-	-	„	100
Indebtedness of Communes	-	„	100	16

The excess on the brachykephalic side of the tax on land (unimproved property) is due to the greater area included. The dolichoid group is evidently the more active, wealthy, commercial, industrious, luxurious; financial needs are greater, whence larger public debts.

This table may be tested to some extent by the following statistics from Italy:—

		Northern Italy Provinces.		Southern Italy Provinces.	
		11 most br.	13 most dol. (or least br.).	11 most br.	10 most dol.
Population		4,680,000	4,516,000	5,022,000	5,038,000
Taxes on Land	- -	100	64	101	81
„ Buildings	-	100	131	85	88
„ Personal Property		100	149	58	59
Inheritance Tax	- -	100	97	50	59
Registration Fees	- -	100	105	87	91
Totals, allowing for differ-					
ences in population	-	100	113	71	68
Totals, excluding Land-tax		100	132	64	67

Here the comparison is fairer in one respect ; every one of these divisions, except the third, contains several large towns. The dolichoid element in the second consists largely of the northern dolichoblond race ; that present in the fourth, and to a less extent in the third, is pretty purely Mediterranean ; and in this race the leading qualities of the dolichoblond are not supposed to be highly developed. The attachment of the brachykephal to the land seems to be shadowed in the large proportion it bears in his districts to other kinds of property.

Ammon finds that a natural selective process continues to work among citizens, so that the longheads gradually rise towards the higher social strata. Thus in the higher schools the proportion of longheads is very large.¹ And in thirty intellectual men, members of a scientific society, he found an index of 80·8, while twelve of the most distinguished yielded one of 79·6, the lowest mentioned in his book. These low indices result from increased length, not from decreased breadth.

Against this observation of Ammon's must, however, be placed that of Houzé of Brussels, who found that in thirty

¹ It will be remembered that Dr. Venn's extensive observations showed that first-class men at Cambridge were longer-headed than second-class men, and they again than the simple pass men and the failures. Muffang of St. Briec finds that in schools the dolichoids do best on the modern side, the brachys on the classical. And there is some other evidence for this.

men of good intellectual standing the heads were broader on the average by two or three degrees than those of the lower classes in that city. This part of the subject requires more elaboration.

Athletes (fifty-two members of athletic clubs) were a selection in the dolicho-blond direction, *i.e.*, in the direction of the true old Germanic type, the average index in ten purely citizen athletes being only 80·5, and their heads being absolutely as well as relatively long, criminals (sixty at Freiburg), were a selection to some extent in the other direction, but not so distinctly. The kephalic index was normal, but the head small, the skin dark, and brown eyes and brown hair in excess.

Another class studied by Ammon was that of clerical students. He found them decidedly short-headed; but as they are drawn for the most part from the peasantry this is what might have been expected.

Phenomena of colour have been almost or quite as carefully studied by Ammon as those of headform, but not with quite so distinct or valuable results. In general the numbers are not large enough to give anything better than a moderate degree of probability. On the whole, the blond type increases relatively with city pedigree and the brown type decreases, but this is more distinct with regard to the eyes than to the hair. The pure blond type of Virchow (blue eyes, blond hair, fair skin) certainly increases in the conscripts; so it does in the "Realschulen," while dark hair, though not dark eyes, increases in the gymnasia. These results are difficult to reconcile with the facts which I have given in a former paper, as to the deeper coloration of both eyes and hair in German cities compared with the surrounding rural districts. Perhaps both the fair Germanic and the dark Mediterranean type (which latter, however imported, does seem to be present in the Rhine Valley) may be more favoured than the brown brachykephalic one, in the struggle for survival as carried on in these cities, while mixed forms, resulting from the crossing of incongruous types, together with neutral eyes and round heads, are more likely to be worsted and thrown out.

It must never be forgotten that the conditions under which problems of this sort arise in England are quite different, owing to the comparative weakness¹ in this country of the brachykephalic, and the greater strength of the Iberian or dark long-headed element. The same may be said of Spain and Portugal and Southern Italy; while in Scandinavia the dolichoblond type prevails still more exclusively. Hence most of the anthropo-sociological investigations hitherto undertaken in this country have been more or less barren, at least those based on the diversities of the kephalic index. Thus I got for the citizens of Bristol an index identical to a fraction with that of the natives of the surrounding counties; nor do I find that the higher or educated class differs materially, *in this particular point*, from the proletariat.

Returning, however, to the middle zone of Europe and the Alpine race or type, we find further and very curious evidence to the migratory instinct of the dolichoid element. Thus De Lapouge lays it down that "the kephalic index of the children of parents belonging to two different districts is lower than the mean between the indices of these districts"; or, which amounts to the same thing, that "the dolichokephal members of a community are more apt than are the brachykephal members, to choose their spouses outside of their own birthplace". This he tests by comparing the index of subjects whose parents were born in one and the same canton with that of subjects whose parents were born in different cantons. These he calls cantonaux and intercantonaux. In the Herault, with a large amount of material, he finds the index of the former to be 81·5, that of the latter 79·8. A number of small series in other parts of France give analogous results; and the law seems to be true of departments as well as of cantons, two small series of them giving contrasts of 82·9-81·1 and 83·2-82·3.

All the processes and tendencies we have been reviewing, and others which have been left unmentioned because less important or less certain, may be grouped together

¹ De Lapouge says "absence," but that is too strong a term.

under one law, which Closson has most fitly styled "the Law of Lapouge," *i.e.*, that of the greater activity of *Homo Europæus*.

Whether we can conceive of the ultimate results of all this as being in agreement with the Darwinian law of the survival of the fittest, unless we are careful to ascribe only the absolutely correct meaning to the word fit, is a little doubtful. The dolichoblond man was developed, we believe, in the course of a long and arduous struggle against the hostile powers of nature, as well as against his fellow-man. Beowulf and Siegfried and Hercules were his ideals. Now perhaps he may not be quite so much needed. Most of the processes we have reviewed may seem to turn to his advantage, but this is not so in the long run. Most of the great things that have been done in the world, it is said, have been done by him;¹ but in doing them, or in consequence of doing them, he and his progeny are very apt to perish. How few descendants can be found of great soldiers, travellers, discoverers, inventors, poets. The higher and more enlightened classes in communities, the producers and assimilators of new ideas, have repeatedly in the course of history been swept away or decimated, while the proletariat survived. Thus the noble Greek race, which was long-headed and largely blond, has now but few and doubtful representatives; the Ostrogoths, a people evidently of great capacities, almost wholly perished; the nobler strains of the Irish people perished or emigrated in the seventeenth century. And now-a-days, in the cities of France and central Europe, the dolichoids seem to melt away, to give place to fresh strains of brachykephalic peasant blood. The fittest, who survives, is therefore the quiet, unambitious, commonplace thickhead, who remains at home and tempts no dangers. It may be that when wars have ceased to be and there are no more regions to explore, and perhaps fewer scientific realms to conquer, this may be the happiest as well as the fittest, *i.e.*, the best adapted class of man. It certainly seems most suited to a socialist organisation.

¹ Napoleon, however, though blue-eyed and rather blond than dark in hair, was decidedly brachykephalic.

Or again, as cities become more healthy, and rural districts less peopled, it may be that the type we call Mediterrean or Iberian, the long-headed dark type may, as Ammon seems to expect, acquire a numerical preponderance.

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JOHN BEDDOE.

THE ZEEMAN EFFECT AND DISPERSION.

A GOOD many years ago, as the years of scientific discovery in the nineteenth century are counted, Faraday observed an action of magnetism on Light. He observed that when a beam of plane polarised light is transmitted through a transparent solid along the direction of a magnetic field in which the solid is placed, the plane of polarisation is rotated. This action of matter when magnetised upon light may be described as due to its transmitting a beam of right-handed circularly polarised light at a different rate from a left-handed beam. This difference of rate of transmission is an extremely small part of the whole rate. The rotation of the plane of polarisation is a very delicate test for a difference between the rate of propagation of the right- and of the left-handed beam. For example, we may suppose a substance such that, after passing through one centimeter of it in a certain magnetic field, the plane of polarisation is rotated through a right angle. It would require a *very* strong magnetic field to produce this effect with the most active transparent body known. Yet the change in the rate of propagation involved would be very small even in this case. A rotation of the plane of polarisation through a right angle would require one of the circularly polarised beams to gain one-half wave length over the other in going through a centimeter. As there are some 17,000 wave lengths in a centimeter of air and in the dense media that are magnetically active about 30,000, it is evident that a gain of a half wave length per centimeter means a difference of rate of propagation of only one 60,000th part of the velocity. In most cases the rotation per centimeter is very very much less than this, so that in these cases the phenomenon is due to a very much smaller action between the magnetised medium and light.

It is to be observed that this action is an indirect one. It is not an action of magnetic force directly on light. It is an action of magnetic force on matter, and of the

magnetised matter on the light. Magnetic force changes matter in some way owing to which the magnetised matter transmits right- and left-handed circularly polarised beams at different rates along the direction of magnetisation. Without matter there is no known action of magnetic force on light. Another thing to be observed is that the action is negligible for very long ether waves and only becomes of importance for the short waves which are in the neighbourhood of the visible part of the spectrum. The phenomenon is much more closely allied to dispersion than to refraction. The longest ether waves are not propagated at the same rate in matter as in free ether, they are largely refracted. Long ether waves are, however, all propagated at nearly the same rate. The differences of rate of propagation that produce the phenomena of ordinary dispersion are only of importance in the case of the short waves which are in the neighbourhood of the visible part of the spectrum. In the case of both the effect Faraday observed, and of dispersion, the action is roughly inversely proportional to the square of the wave length, thus leading one naturally to expect that they may be allied phenomena.

If, with this clue, we were to seek for some explanation of the Faraday effect we should study the various theories that have been propounded to explain dispersion. The oldest of these explains dispersion by the hypothesis that the distances between the molecules of matter are comparable with the lengths of the waves of light. In the case of wave lengths, for which this is true, there must exist dispersion phenomena, and until some rough estimate had been made of the distances between molecules of matter there seemed good reason to suppose that the phenomena of ordinary dispersion were due to this cause. The theory explained the laws of ordinary dispersion as well as could be expected from a rough theory, which took no account of the structure of the molecules themselves. Now, however, it is known that the distances between the molecules in transparent solids is a great deal too small to explain ordinary dispersion. Dispersion due to this cause must be concerned with waves of very much shorter wave length

than any that have been satisfactorily measured ; perhaps some phenomena may be observed in reference to Becquerell or X rays depending on their wave lengths being comparable to the distances between molecules in solids. Hence it is not in the direction of this theory that we should expect to find an explanation of the Faraday effect.

The theory of dispersion that seems to explain very satisfactorily the phenomena is one that attributes it to a comparability between the frequency of vibration of the light and of the atoms of matter : to a resonance of the matter to the vibrations of the light passing through it. It is not very easy to put the dynamical basis of this theory in general terms in a simple manner. The phenomena of forced vibrations are not familiar to most persons, and it would take a long explanation to bring the matter clearly before those not already familiar with these phenomena. The part of the phenomena on which ordinary dispersion depends might be illustrated by the following experiment. Suppose a long chain stretched between two points. We are all familiar with the way in which a wave can be propagated along such a chain. The rate of propagation is independent of the length of the wave so long as it is not comparable in length with the links of the chain. If the links have masses of lead attached to them the result will be that these long waves will all be propagated more slowly than before, but all the same amount more slowly. This may be taken as analogous to the slower propagation of light in transparent matter than in free ether. It will complicate this simple result if the leaden masses, instead of being simple masses each firmly attached to a link, are each made up of two masses fastened together by a spring so as to be capable of vibrating independently of the links. The effective inertia of these double masses will be the same as if they were rigidly fastened together so long as the vibrations of the links are very slow compared with the independent vibrations of the double masses due to their parts being connected by springs. When the links vibrate slowly the double masses will move together, and there will be very little motion of one of their parts relatively to the

other. When, however, the frequency of vibration of the links is at all comparable with the frequency of relative vibration of the parts of each double mass these parts will be more or less violently agitated, and there will be a considerable relative motion of the parts of each double mass with a straining of the spring connection between them. The effective inertia of the double masses in this case is quite different from what it is when there is no internal relative motion of their parts. The general result is that when the frequency of vibration of the links is less than that of the double masses a wave is propagated more and more slowly the more nearly the two frequencies approach coincidence. When the frequencies coincide ordinary wave propagation ceases in the theoretical case, as all the energy is spent in increasing indefinitely the internal vibrations of double masses. In considering the case of ordinary dispersion we need not look into the matter further. Ordinary dispersion consists in the propagation of waves of great frequency being slower than that of waves of smaller frequencies, and it is easy to see how the above-mentioned analogy illustrates this phenomenon. We must suppose the atoms of matter immersed in the ether capable of internal vibration at a frequency somewhat greater than that of the light we are studying, and then we might expect the waves of frequencies approaching those of the atoms to be propagated more slowly than waves of slower frequencies which do not agree so nearly with that of the atoms. Two things will determine the amount by which the matter will alter the velocity of propagation of the light. One will be the amount of interconnection between the matter and the ether. We might naturally expect some kinds of matter to be only very slightly acted on by the vibrations of the ether in which it is immersed, while other kinds of matter would be much acted on by these vibrations. The reaction of the matter on the ether and on the rate of light propagation would be in the first case small, and in the second case large: the first kind of matter would generally be more highly refractive and dispersive than the second. The other thing that will determine the amount of dispersion

will be the nearness of the frequency of vibration of the atom to the frequencies of vibration of the waves we are studying. If the atom frequency is only a little greater than that of the waves its effect upon them will be great, and the velocity of propagation of the waves will be slower than if the atom vibration were of much greater frequency than that of the waves.

That atoms of matter can vibrate at frequencies somewhat greater than those concerned in the light we use when studying dispersion is manifest from the spectra they exhibit. The very great opacity of all known transparent substances, even of gases, to ultra-violet light of frequencies of vibration not very much greater than the frequencies usually studied proves that all these bodies are capable of absorbing vibrations of the frequency of these ultra-violet vibrations, and are, consequently, capable of themselves vibrating at these frequencies. There is, consequently, no objection such as could be urged against the other theory of dispersion to this, what may be called, resonant theory of dispersion. In one case the two theories run into one another to some extent. When masses are regularly distributed through a medium, we may consider the medium between any pair of masses as capable of independent vibration : it could, in general, vibrate with a node at each mass and a set of intervening loops. The slowest of these rates of vibration would be comparable with that of a wave whose length was double the distance between the masses so that we might consider the phenomenon as due either to a comparability of the wave lengths to the distances between the masses, or to a comparability of the frequencies to the frequencies of vibration of the medium between the masses. The subject does not seem of much immediate interest, however, as phenomena depending on these very short waves have not been sufficiently studied to be capable of discussion in connection with a theory of this kind.

It is evident, then, that anything which alters the vibration frequencies of the atoms of matter must alter dispersion. Altering the density of matter will also, of course, alter the rate of propagation of light : it will alter the grip that

the matter has on the ether. Hence there are two ways of altering the rate of wave propagation, one by altering the amount of connection between the ether and matter, and the other by altering the frequency of vibration of the matter. For example, by increasing the density of a gas we must reduce the velocity of propagation of light through it both because there is more matter acting on the ether and because, as has been shown, the rate of vibration of the molecules is reduced and thus the frequencies of vibration of the ultra-violet vibrations that control dispersion are brought into closer coincidence with the visible radiations and this makes the rate of propagation of these latter slower. This latter effect has not been observed owing to its extreme minuteness. There may also be other effects on the waves due to other changes in the vibrations produced by the increased density. For example, it has been shown that there are certain absorption bands in oxygen whose intensity depends on the square of the density of the gas. They are consequently, in all probability, due to vibrations which are only executed during the collisions of the molecules, and if the vibrations that control dispersion are in any sensible degree of this character, there may be a corresponding reduction of velocity of propagation of light through oxygen depending on the square of its density.

One of the best-known methods of observing these changes in velocity of propagation due to changes of structure is by observing with polarised light the crystalline structure produced by straining transparent solids. In this case, as in that of crystalline structure in general, the rates of propagation of vibrations executed in different directions are different. This may be due either to the interconnection of the matter and ether in different directions differing, or to the frequencies of vibration of the molecules in different directions differing, or to both these causes. On account of the want of sharpness of the absorption bands in solids it would not be easy to observe the probably very small change in their position due to straining a solid, so that it does not seem probable that we can in the near future distinguish between the two causes in the case of solids as we

seem already almost in a position to do in the case of some gases. The dichroism of some crystals makes it possible that some at least of their double refraction may be due to a difference of frequency of the vibrations that control dispersion in different directions.

The question thus arises whether we cannot, in accordance with their principles, explain the differing rates of propagation of right- and left-handed circularly polarised light that Faraday observed to exist in matter in a magnetic field. This Faraday effect might be due to one or other of two causes. Either the magnetic field might cause a greater interconnection between the matter and the ether for one of the circularly polarised vibrations than for the other, or it might cause the molecules that are vibrating round in one direction to have their frequency increased while the frequency of those vibrating round in the other direction was decreased. Either of these causes would explain the Faraday effect. The first of these is the kind of effect we might expect to be produced by that action of magnetic force on matter by which it is supposed to orientate the molecules. This is the generally accepted cause of paramagnetism and the Faraday effects in paramagnetic bodies are sufficiently different from those in diamagnetic bodies for it to be probable that their cause is somewhat different. From this point of view it would seem natural to conclude that in diamagnetic bodies magnetic force had the power of altering the rates of vibration of the molecules and causing the right-handed circularly polarised component rotating round the lines of magnetic force to be executed at a different frequency from that of the left-handed component. It is well to consider these right- and left-handed circular vibrations as components of a more general vibration because it is improbable that the actual vibration in any one molecule is an accurately circular motion. It may also be worth while pointing out that the result of compounding two equal circular vibrations in the same plane, but of slightly different frequencies is to produce a linear vibration whose line of vibration rotates in the plane of the two circular vibrations. Hence we might describe the effect

we have supposed magnetic force to have on the vibrations as being a rotation of all linear vibrations round the lines of magnetic force, or a precessional rotation of the axes of the vibration, in the case of non-linear vibrations. Lorentz has pointed out that if the vibrations in matter that are connected with the ether are due to the motions of electrons, this very precessional motion is what might be expected from the disturbance of the orbits of the electrons by the magnetic force. We may then reasonably expect that magnetic force does make the right-handed circular component of the vibrating molecules to rotate at a different frequency from the left-handed component, these components rotating round the lines of magnetic force. Hence, one of these components in the ultra-violet vibration that controls dispersion will become more nearly of the same frequency as that of visible light and so will cause the corresponding circular component of the visible light to be propagated more slowly than the oppositely rotating component,—for this latter will be less in accord with its corresponding vibrations in the molecules when magnetised than when there was no magnetic force acting, and so will be less affected by them. Hence there would seem to be every reason for attributing the Faraday effect in diamagnetic bodies to such a change in the vibrations of the molecules as has been described. When we consider the observed direction of the Faraday effect in diamagnetic bodies we find that in order to explain it by Lorentz's theory we must assume that the connection between matter vibrations and the ether is principally due to the motion of a *negative* electron. This is no doubt a very remarkable deduction from the theory, but some phenomena of vacuum tube discharges seem to imply that the negative electron is more free to move about than the positive electron. This may all be due to some essentially positive characteristic of the matter in our part of the universe. The accumulation of matter of the same kind in one system being possibly produced by its gravitational repulsion for matter of the negative character.

And now what independent evidence is there that magnetic force can influence the vibrations of molecules?

Faraday looked for this and Zeeman found it. The direct observation of an alteration in the vibrations of molecules by magnetic force is one of the most interesting and important observations made of recent years. It is most interesting and important for several reasons. It enables us to study the internal vibrations of molecules by modifying in a simple way the conditions under which these vibrations are executed. The laws of action of magnetic force are so well known that we can be fairly sure how it acts on the molecular motions and from a study of the effects produced we can already make inferences as to the nature of those motions in the molecules to which the spectral lines are due. For example, the hypothesis has received confirmation that the lines are due to the motions of electric charges, and there seems reason to suppose that these electric charges are accompanied in their motions by only a small part of the mass of the molecule. Further, we are now able for the first time to produce polarised radiations from the molecules of a gas. Hertz showed how to produce polarised radiations from electromagnetic oscillations on conductors, but hitherto attempts to produce polarised radiations from gaseous molecules have failed. This is in itself a great advance in our means of studying these vibrations : it simplifies the problem to be studied by analysing it into components. We know that different lines in each spectrum have different physical characteristics such as belonging to different so-called series of lines, being reversed with more or less ease, becoming more or less expanded, etc., under changes of temperature and pressure and we are now provided with a further characteristic difference between lines in that they are differently acted on by magnetic force.

The effect of magnetic force on the vibrations of molecules is a complicated one. The apparently simplest effect upon a vibration is to produce the result already described as required in order to explain the Faraday effect and which Lorentz has taught us we should expect to be produced on a vibrating electron. Every simple vibration of a point may be analysed into two circular rotations, one right- and the other left-handed, in a plane at right angles to the magnetic

field and into a linear component in the direction of the field, all these being of the same period. The effect of the magnetic force in the simplest and apparently very common case is to leave the component in the direction of the field unchanged and to make one of the circular components rotate more rapidly and the other less rapidly than when there is no magnetic force acting. The changes of frequency are directly proportional to the strength of the magnetic field, but are independent of the intensity of the vibration of the molecule. This simplest action is what we would expect to take place when the magnetic force acts on a moving electron which is equally free to vibrate in every direction. On account of this important condition, however, we need not be surprised to find more complicated effects produced in the case of a large number of spectral lines. It is very improbable that the molecules of most gases are so symmetrical that all vibrations in every direction are equally possible, and, as a matter of fact, very complicated effects have been observed in the case of a large number of lines. The effect of magnetic force in this simplest case is to make the axis of the orbit of an electron rotate round the line of magnetic force. We might expect a disturbing force to produce other changes in the orbit, in general; such, for example, as causing the inclination of the orbit and its eccentricity to change. Actions such as this would produce complications in the spectra, as has some time ago been pointed out by Dr. Stoney.

These theories as to light vibrations being due to simple harmonic vibrations of electrons are, however, almost certainly only provisional. They require the forces acting on the electrons to be directly proportional to the distance from their positions of equilibrium. It is unlikely that electrons can be subject to such forces and their vibrations are much more probably of the nature of perturbations of orbital motions executed under quite other laws of force. For example, the rotation of the lunar nodes is a vibration of, a system, the earth, sun, and moon, which is controlled by forces varying inversely as the square of the distance and is one whose period is almost independent of the eccentricity of

the lunar orbit although the amplitude of the radiation such a rotation might emit might be directly proportional to this eccentricity. The whole question is very complicated. It practically assumes that the fundamental motions in the molecules are immensely more rapid than any of those we deal with in light vibrations, and that these latter are merely perturbations of the fundamental motions. It is well to keep these things in mind, although we are to all appearance so very far from any satisfactory explanation of it all, because it points out the direction in which to look for an advance in our knowledge. That we know so little and see so little ahead in these fundamental matters may be disheartening, but it shows how very important any advance in our knowledge of molecular motions is and should encourage us to a study of even minute effects such as Zeeman has observed.

In the simple case referred to above, the radiation emitted under the action of magnetic force, when studied by a spectroscope, has the following characteristics: the radiation emitted along the lines of magnetic force consists of two circularly polarised beams, one right- and the other left-handed, vibrating at slightly different rates. This is exactly what might be expected to be emitted from an electron moving in the way already described. The component of the vibration along the lines of force would not give rise to any sensible disturbance at a distance along that line, and the two circular components of its vibration in the plane at right angles to the force would produce the observed circularly polarised beams. The effect of the magnetic force on a spectral line which is thus simply affected is, when viewed along the lines of force, to double the line, one of the components being right-handed and the other left-handed circularly polarised light.

The radiation emitted in directions at right angles to the magnetic force is most simply described as consisting of three beams. One due to the component along the lines of force whose frequency is unchanged, and which produces a plane polarised beam and resulting line in the spectrum in the same place as when there is no magnetic force acting; and two other beams due to the two circular components

whose frequencies are one greater and the other less than that of the unchanged vibration. The radiations emitted by these circular components in the plane of their motion are plane polarised in a plane at right angles to the unchanged one. The effect observed in the spectroscope is the production of two lines, one on each side of the unchanged one, and both plane polarised in a plane at right angles to that of this unchanged one. Further, on considering the direction of rotation of the circularly polarised beams emitted along the lines of magnetic force, it can be shown that the beams must be principally due to the rotation of a negative electron.

All this direct observation of the effect of magnetic force on the vibrations of molecules confirms the theory already expounded as to what ought to take place in order that we might explain the Faraday effect by a resonant theory of dispersion. It proves that there really exists the action of magnetism on the vibrations of the molecules that that theory postulated, and consequently proves that here is a *vera causa* for a Faraday effect. All that is further required is to show that it is a sufficient cause for the observed Faraday effect. Unfortunately the data required for this confirmation of the theory are not available. We should know, in the case of at least one gas, the amount of the Faraday effect, of the Zeeman effect, and in addition the law of dispersion by the gas sufficiently accurately to calculate the frequency of the principal absorption that controls its dispersion. The Zeeman effect and Faraday effect have not been measured in the same gas. Besides this, the Zeeman effect is not the same for the various lines in a single spectrum, and we know no law for calculating its effects on one line from observations on another. What we require in our calculation is the Zeeman effect on the absorption band that controls dispersion, and this has not been observed in any gas, the band being far up in the ultra-violet. For these reasons there is little prospect of any except a very rough comparison between theory and experiment being possible for some time. When we make a rough estimate of the possible Faraday effect from the observed amounts of the

Zeeman effect in the substances already studied and from the observed dispersion in air, we find that it is of about the amount that has been observed for the Faraday effect in air. So far, then, it appears probable that the whole cause of the Faraday effect is to be looked for in each substance in a Zeeman change of the free vibrations of the molecules of the substance by the action of magnetic force, this change in the free periods reacting through their resonances on the rate of propagation of the circularly polarised components of light passing through the magnetised medium.

A theory on similar lines may be worked out to explain the Kerr effect and the Hall effect: this latter being an action on electrons moving continuously in one direction, not on simply vibrating ones. This has been gone into by Mr. Larmor in his valuable papers on Electromagnetic Theory in recent volumes of the *Phil. Trans.*

A very interesting and fundamental question is raised by all this theory as to the cause of the Faraday effect when we compare it with former theories as to its cause. Former theories all attributed it to a gyrostatic action accompanying propagation of waves along lines of magnetic force. The illustrations of magnetic rotation of plane of polarisation have all been derived from the propagation of waves through media in which rotating gyroscopes played the part of the magnisation of the matter. It is, in fact, impossible not to fall back directly or indirectly upon a rotation when we look for an explanation of the Faraday effect upon dynamical principles. Yet, where, in the theory here profounded, is any rotation postulated or gyrostatic action involved? The action of magnetic force on a moving electron, which is postulated to be the same as on an electric current, does not appear at first sight to involve gyrostatic actions. Yet the whole theory given depends on this action and on dynamical effects which do not in any way involve gyrostatic effects. Mr. Larmor, for example, supposes magnetic force to be due to an irrotational flow in the ether and an electron to be a singular point surrounded by a peculiar condition of twist. Where in this is

any gyrostatic action postulated? Apparently nowhere. When, however, we look further into the matter, we find that even in Mr. Larmor's theory a gyrostatic effect is introduced in order to explain the properties of his ether upon ordinary dynamical principles. His ether is supposed capable of elastic reaction to *absolute* twist. This is only to be explained on ordinary dynamical principles by attributing to the ether gyrostatic properties due to some of its parts being in rapid rotation. It would appear, then, that, if not directly, at all events, indirectly, the phenomenon of the rotation of the plane of polarisation of light by magnetised media is rightly considered as involving gyrostatic properties in the ether and so far tends to confirm the hypothesis that the properties of the ether may be due to the rapid rotation of the parts of a fluid.

GEO. FRAS. FITZGERARD.

SOME RECENT WORK UPON MUSCLE AND NERVE.

THE appearance of an excellent English translation of the work of Prof. Biedermann affords an opportunity both for a brief notice of the book and for directing attention to some recent work in connection with the physiology of muscle and nerve.¹ The title of the book is somewhat misleading since the author comprises under the heading "Electro-physiology" not only the electromotive changes which under certain conditions present themselves in living tissues, but all the excitatory phenomena capable of being called forth by the action upon these of electrical currents.

Now the functional characteristics of such structures as muscle and nerve have been chiefly studied by acting upon these tissues through the medium of definite stimulating agencies and noting what changes are thus evoked. For this purpose the stimulating agency generally employed has been that of a brief electrical current and it thus follows that the study of the action of currents comprehends almost all the known phenomena of muscle and nerve activity. Prof. Biedermann's book, although entitled "electro-physiology," deals in reality with a far more extensive range of phenomena than that which the name suggests, and the scope of the work would be more correctly indicated by the phrase "phenomena of the excitable tissues". The treatise is the most comprehensive and most valuable contribution to the literature of the subject which has yet appeared, and since it deals with fundamental questions it acquires an importance in connection with the study of vital phenomena which cannot fail to be appreciated by all those who are interested in biological science.

It may be asked on what grounds the term "fundamental" is here used. Its employment will be justified if we briefly consider what are the essential characters of

¹ *Electro-physiology*, by W. Biedermann, translated by Frances A. Welby. London: Macmillan, 1898. 2 vols.

vital activity as displayed by those methods of inquiry now utilised by physiologists.

Living tissues are characterised by possessing peculiar properties, one of which is that of undergoing changes, the real causation of which is unknown to us. The changes are, however, readily recognised since they display themselves as physico-chemical effects of a definite and distinctive character. Each tissue shows a group of such effects which becomes more special as the tissue itself becomes differentiated in structure. The display of such definite changes is the sign to the physiologist that the tissue possesses vitality; the potentiality for such display may be termed its capability of functional activity, or, in short, "functional capacity".

This functional capacity is swayed by such conditions as the chemical nature of the environment, etc., it is thus not a constant quantity but one which varies, now increasing, now diminishing.

Its complete abolition beyond all power of recovery constitutes death, and thus the essence of the mysterious and unknown quantity termed life, is the possession by the tissue of this capacity for undergoing change of a definite character. It is, therefore, the possibility of the appearance of a new set of physico-chemical phenomena in such tissues as muscle and nerve which constitutes the fundamental characteristic of the living as distinct from the dead state. In these tissues the condition of activity can be evoked by producing physico-chemical changes in their substance. The evoking change acts in such cases, not as the causative factor of the subsequent active phenomena, but as an agent which releases, and thus initiates, a whole series of more subtle unknown chemical disturbances, the gross result of which we recognise as distinctive. Its action is analogous to that of the electrical current which fires a submarine mine and thus initiates an explosion. The releasing or initiating agency, in the case of the tissue, is termed the exciting cause or the stimulus, whilst the subsequent effects evoked by this are defined as those of the "state of excitation". Finally, the possession by any tissue of susceptibility to be brought into the state of excitation

on receipt of a stimulus is termed its "excitability," and, in proportion as this is more easy of achievement, so the tissue excitability is higher.

Muscular and nervous structures are those which show in the most marked degree excitability, and they are further characterised by the circumstance that the state of activity when evoked, is accompanied by phenomena which are readily recognised, such as movement and electrical changes. In plants the leaves of *Dionæa muscipulata* are similarly endowed with high excitability and an easily recognised state of activity. It is for this reason that the term "excitable" has been more or less used as a qualification for these tissues, but it is evident that the phrase "excitable tissues" must on philosophical grounds have a far wider scope, and may be taken to include all living structures.

All possess a functional capacity for change, and in all this change must be initiated by some physico-chemical event, which is therefore the releasing agency or stimulus. Even when the resultant effect is one of such slow development as the division of a cell, the manufacture and discharge from the cell of new substances and all the varied phenomena which constitute growth, the functional activity thus gradually displayed must be regarded as due to states of excitation evoked in excitable structures by appropriate although at present little known stimuli.

The capacity for passing into new physico-chemical moods and the property of being forced to perform this passage in response to definite stimuli, are thus fundamental characters of living as distinct from dead matter. Those tissues which show the phenomena best, such as muscle and nerve, enable us to grasp more completely than any others upon what conditions the possession of these fundamental characters depends. This is the real importance of the study of the excitatory phenomena of muscle and nerve, since we learn not merely their own individual traits, but obtain results which serve as sign-posts, guiding us along well beaten tracks towards the investigation of widely removed living structures.

Muscle and nerve possess in a marked degree other

fundamental vital characteristics. An excitable tissue passes into a new active state on receipt of a stimulus ; the activity thus evoked may persist for a considerable time or may rapidly pass away, but in either case it is clear that a complete return to the resting inactive condition may accompany the subsidence of the state of activity. Such a restoration of the tissue, the remaking of the particular molecular combination which existed before the upset brought about by the stimulus, is a striking feature of the activity of muscle and particularly of nerve, hence these structures enable us to examine the conditions which influence this phase of the living cycle more completely than any others. The process is of fundamental importance, since it is the antithesis of that involved in the state of excitation, and both processes are present in every living tissue. The stimulus acts upon one of these, and it develops such preponderance that it masks the opposite one. It is the peculiar merit of Prof. Biedermann's book that this fundamental conception of living tissues so strongly urged by his master, Hering, is kept before the reader.¹ In such a tissue as muscle it is possible to ascertain many essential characters of the state of restoration, and to determine further how far the disappearance of the state of activity is a necessary prelude for the reinstallation of the resting functional capacity. Will the tissue respond to a second stimulus whilst the excitatory state evoked by the first is present? It is well known that striated muscle will respond in this way, so that even when a contraction due to one stimulus is present it will contract again. Hence in this tissue a portion only of the material whose change transforms the resting into the contracted muscle is affected by the exciting agent ; a second stimulus can initiate a further transformation in another portion, and so on with a third and a fourth stimulus until the total mechanical effect reaches four times that due to the first response.

On the other hand this trait is by no means universally present, and even an allied structure like cardiac muscle

¹ The short account of Hering's theory of the functions of living matter, published in 1888, has been translated into English by Miss Welby, and appeared in *Brain*, vol. lxxvii., p. 232 (1897).

does not exhibit it. Here the explosive disturbance of the active state is accompanied by diminished susceptibility to respond to a stimulus, in other words, diminished excitability. The study of the precise conditions which affect the restoration of excitability and of functional capacity might be illustrated by many instances drawn from the physiology of muscle and nerve. It will be sufficient in an article of this character to indicate that the whole subject is treated in the most suggestive manner by Prof. Biedermann, and should afford valuable help to any who desire to ascertain how far modern physiology has succeeded in the investigation of such a fundamental vital attribute as the property of returning from the active to the resting state.

One other point of general interest is brought into prominence by the study of muscle and nerve now before us, namely, "functional capacity".

From the above remarks it will be gathered that "functional capacity" implies the capability of the tissue to undergo a change resulting in definite new effects those of activity, whilst "excitability" implies the susceptibility of the tissue to enter upon this change on receipt of a stimulus. At first sight the two phrases seem to be identical, but a little consideration will show that they are not, although it must be admitted that the significance of their separation has been only grasped during the last decade, mainly through the work of Hering and of Biedermann. The measure of functional capacity is the comparative amount of active display which the tissue is capable of showing, the extent and force of its excitatory explosion; the measure of excitability is the comparative ease with which it may be forced to enter upon its functional active state. Nerve is the most excitable of all tissue, and thus affords the best instances of the distinction just referred to. It may be rendered less excitable, but of increased functional capacity, by the passage of a weak voltaic current, this change occurring at the point of entry, or anode, during the current flow. On the other hand, it is more excitable but of diminished functional capacity at the point of exit, or cathode, of this current. Many other instances might be

cited of conditions which change excitability in one direction and functional capacity in the other; and since there are also instances in which the two both vary the same way, it is clear that the molecular foundations of excitability and functional capacity are not so firmly linked together that a change in the one must produce a corresponding change in the other.

We may picture to ourselves the living metabolism in any one region of a tissue, such as nerve, as consisting on the one hand of the tightening, and on the other of the loosening of complex molecular bonds. An apt illustration is afforded by the stability of a house built of cards, and having such a roof, that it is in itself an unstable structure. A slight shake of the table (the stimulus) can secure the downfall of such a house, and this downfall we may liken to the state of excitation.

Its degree of instability we may term its excitability; the more this is increased, the more readily is the fall evoked. On the other hand the magnitude of the fall depends upon the number of cards which constitute the house, and as these are more numerous the capacity of the house to produce a large downfall is obviously increased.

Functional capacity is measured by the magnitude of the state of excitation, hence in the illustration functional capacity will be increased in direct proportion to the number of cards constituting the house and capable of being overthrown in its fall.

Now it is possible to make the house more stable by buttressing its sides with cards; it is now less readily upset (less excitable); on the other hand, if it should be upset the number of cards involved in its fall is increased (increased functional capacity). But it is also possible to render it more stable by removing those top-heavy cards which give to the roof its inordinate size and weight. Under these circumstances it is more stable (less excitable), but there are fewer cards to fall when it is upset (diminished functional capacity). The illustration will not bear being pushed, unless we introduce a human agency for the process of restoration, the rebuilding of the house

after its downfall, but it serves to illustrate the distinction between the capacity of a tissue for the active state and the readiness with which it enters upon that state. Although these two aspects, functional capacity and excitability, have been only conceived as distinct through the study of the phenomena of muscle and nerve, it is becoming obvious to all those who have worked upon the subject that the conception of their distinction is probably applicable to all living tissues, and thus we are presented with a striking instance of the use of the study of muscle and nerve in enlarging our views upon vital phenomena as a whole.

Prof. Biedermann treats in his book of other excitable structures possessing definite and easily recognisable excitatory states besides muscle and nerve; several chapters are devoted to the electromotive changes, in plants, in epithelial cells, in the retina and in electrical organs.

It would be undesirable to review in any detail the large amount of data which he has compressed into the pages which deal with these subjects; the subject-matter is somewhat complicated and special, but is admirably given, the sole fault which the reader is likely to find being *l'embarras de richesses* owing to the amount of data brought forward. Up to the year of its publication in Germany (1896) the work may be considered as a comprehensive account of the present position of our knowledge. In physiology, as in every growing science, two years implies a considerable extension of previously existing experimental data and the introduction of entirely new points of view to explain the results of recent inquiry. This has been the case with the physiology of the excitable tissues. It would be impossible in the present article to indicate the many different lines along which advance has taken place during the last year or two. The present writer has therefore selected two instances which his personal knowledge of the subject-matter has led him to regard with special interest, the one being taken from the physiology of muscle and the other from that of nerve.

The predominant feature of the state of excitation in muscle is undoubtedly the familiar mechanical change which

constitutes its contraction. It is well known that the phenomenon may be studied in the excised muscle of a recently killed cold-blooded animal such as the frog, for many hours after the removal of the muscle from the body; a rapid shortening or twitch occurs in such excised muscle when a single stimulus, preferably an induced current, is applied either to the muscle or the nerve supplying it. The study of the muscular response upon its mechanical side received a notable advance fifty years ago by the application of the graphic method devised by Helmholtz. This involves the attachment of one end of the excised muscle to the short arm of a small lever; since the other end of the muscle is immoveably fixed by a suitable holder, the shortening present during the twitch must move the lever and a longer arm of this can by appropriate arrangements be made to record its displacement upon a blackened surface moving at a known velocity. The record thus obtained is known to all physiological students as the muscle curve; since the rise of the lever with its subsequent fall is the agency which has actually written the curve, the curve itself indicates the successive positions which the lever has assumed at different periods of time.

The influence of various conditions upon muscular activity have been studied by obtaining such curves and noting what alterations are now present in the records. In this way the intensity of the stimulus, the temperature of the muscle, the alterations due to increasing mechanical load, the effects of previous muscular activity, of chemical substances and of many other agencies have been ascertained. In view of all this work it is a little singular that a point of prime importance involving the meaning of the record itself should have only come to the front recently. The muscle curve is the history of the movement of the lever to which the muscle is attached, and obviously this derives its value on the supposition that we may deduce from the movement of the lever, the mechanical change which occurs in the muscle. Does a light lever of the type described faithfully follow the muscular shortening? Is it a reliable historian or has it

a personal bias which garbles the account it gives of the event? As in historical so in these physiological inquiries, suspicion is cast upon the veracity of a writer by the fact that another account of the same event written by a different hand differs from the first in some important particular. Such a second record is obtained when the muscle is attached to a lever of essentially different construction. In that just described the lever always pulled with its small weight upon the muscle: the condition may be thus described as "isotonic" and the curve obtained by it as an "isotonic muscle curve".

But it is a familiar experience that muscles during contraction not only shorten in length but become tense. This stiffening is as much a mood of the mechanical response of muscle as is the shortening and may be communicated to a lever by a very simple device. The muscle is made to pull against a spring, preferably a steel wire fixed at one end and capable of torsion at the other. The torsion of the spring is always counteracting the mechanical tension of the muscle, if the latter increases the former must do so too. The slight movement of the steel wire, if recorded, will indicate any alteration in the amount of the torsion and as it may be easily arranged to move a fine lever, a magnified record of the change can be obtained upon a travelling blackened surface. The shortening of the muscle when it twitches under these conditions is now so little as to be insignificant; its length is kept approximately unaltered by the steel wire and it is thus said to be under isometric conditions, whilst the curve which is inscribed by the lever is termed the isometric muscle curve. It is, or rather it professes to be, the history of the development and subsidence of the muscular tension, and we have thus a second independent account of the mechanical change in muscle evoked by a single stimulus.

When the isometric and isotonic curves are compared it is seen that the two accounts differ in one essential particular, for the time relations of the curves are not identical; the isometric curve reaches its maximum from $\frac{1}{100}$ to $\frac{2}{100}$ of a second before the isotonic. The existence

of this discrepancy has been known for some time but has acquired a new interest from the animated controversy which is still going on between Schenk and Kaiser with reference to its meaning. There are at least two possible interpretations of the discord in the results. The first is that the muscular events recorded are not the same; both histories may be correct, but the mechanical condition of tension present when the isometric record is obtained, may be a disturbing factor and thus alter the muscular activity. This view is held by Schenk, who offers a number of experimental results suggesting that increase in the tension of a muscle so changes it that both the development and the subsidence of the state of activity are hurried up; the result must be that the culminating effort due to active contractile stress occurs sooner than it would otherwise do, since it is both accelerated in onset and cut short or inhibited as regards duration. In other words, Schenk regards a mechanical pull as capable of stimulating the functional capacity of the tissue both for passing into the active and for returning to the passive or resting state. This view involves issues of a comprehensive kind; one of which is the assumption of two separate excitatory processes, one that of activity the other that of subsidence, whose presence annuls or inhibits the former. It would be unprofitable to review at length in the present article the arguments for and against the acceptance of such an enlargement of our conception of excitatory phenomena. The absence of such a review may be further excused on the ground that Kaiser's interpretation of the discrepancy is of an entirely different nature. He regards the contractile stress in the muscle as the same, however, recorded; hence, as the muscular event is in his opinion unaltered, the discrepancy must be due to what may be termed the personal bias of one or both of the historians. Does the isotonic curve faithfully indicate the development and subsidence of the contractile stress evoked in muscle by a single stimulus? To ascertain this he has carried out a number of extremely ingenious experiments, the most convincing of which consist in the production of so-called "arrest" curves. These are

obtained by the following simple device. The upward movement of the isotonic lever is stopped by a suitable mechanism so arranged that the stop can occur at any desired interval after the record has begun. If this arrest occurs before the contractile stress of the muscle has ceased the lever will be held up against the stop; if on the other hand it occurs at the moment when the contractile stress ceases, the lever must fall. Now if the stop is arranged so that the lever falls immediately after its upward movement has been arrested, it is found that this fall begins at a point which is situated on the isotonic curve between $\frac{1}{100}$ and $\frac{2}{100}$ of a second before this curve has reached its maximum. Hence contractile stress has ceased although the isotonic lever continues to rise.

A further modification of the method is that of keeping the lever arrested and suddenly releasing it; here the same thing holds good, for if released before the contractile stress is over it will rise, and in this way the duration of the contractile stress may be determined.

The isotonic record is thus, according to Kaiser, the account of an untrustworthy historian, for it records other things besides the contractile stress. These other things are partly the movement of the lever due to its own momentum and partly that due to the passive elastic swing of the muscle itself. The twofold view of the muscle response advanced by Schenk is thus, according to Kaiser, unsupported by data derived from the muscle curves, whatever support it may receive from other sources. The response is the sudden assumption by the muscle of a new condition of elastic equilibrium and the subsidence of this is not a second response to excitation but is dependent upon the new condition coming to an end.¹

Those who are acquainted with the methods employed in physiological investigation are not likely to underestimate the importance of the issues here raised, but even those

¹ The experimental data, together with the theoretical interpretations advanced by the writers concerned, may be found in Schenk's papers, *Archiv f. d. ges. Physiol.*, vols. 50, 53, 55, 61, 63 and 65, and in Kaiser's paper, *Zeitschr. f. Biol.*, vol. 33, etc.

whose work lies in biological or other fields of natural science must realise that since our knowledge of excitable tissues is largely founded upon the study of muscle, a controversy which involves the credit or discredit of the chief method employed for its elucidation, is by no means the least interesting episode in recent physiological science.

As regards nerve physiology the work of chief interest to the present writer is that which involves the study of the electromotive effects which accompany the presence in this tissue of the state of excitation. There are no known mechanical, chemical or thermal indications to show that nervous tissue has been thrown into the state of activity. Such a state is inferred in motor nerves from the circumstance that the supplied muscle to which the nerve is distributed responds when this nerve is excited. The combined nerve muscle preparation may be likened to an underground fuse leading to a mine where the successful firing of the fuse is indicated by the explosion of the mine. But since the discovery by du Bois Reymond fifty years ago that the state of excitation in nerve was accompanied by a change in electrical state, it has been recognised that the study of this state is a means, and at present the only direct means, for ascertaining the vital characteristics of the nerve itself. A very large number of different methods have been employed for the purpose, but the majority have all involved the frequent excitation of the nerve by a series of stimuli. This necessity has been due to the circumstance that the electrical change which accompanies the molecular disturbance is, when evoked by a single stimulus, too feeble and too short-lived to affect the most sensitive galvanometer. A vast amount of information has however been obtained from the summed effects of repeated stimulation, the galvanometric indications being under these circumstances both definite and considerable. By means of an ingenious automatic key, the revolving rheotome, Bernstein, Hermann, Boruttau and others have obtained a history of the development and subsidence of the aggregate or total of this multiple effect, and from such data they have inferred the history of each single change. All this is set

forth in great detail in Prof. Biedermann's work, but two extensions of the inquiry by English physiologists are too recent to be included and may be referred to here. The first is that made by Waller.¹ It consisted in recording photographically the galvanometric deflection produced by the aggregate of all the changes when the nerve is stimulated for a short period by a rapid series of excitations. The experiment is repeated over and over again at regular intervals and the resulting deflection caused by each series of stimuli thus occurs again and again and these can be photographed one after another upon a slowly moving sensitive plate. The results are valuable inasmuch as they are records admitting of comparison. They demonstrate that nerve is practically incapable of fatigue and show the extent to which both its functional capacity and its excitability are modified by changes in its environment. The influence of anæsthetic vapours of carbonic acid and carbonic oxide gases, of saline and acid reagents, etc., have been studied in this way; and the method undoubtedly opens up the possibility of determining in a more precise manner than heretofore the important question as to the mode in which chemical and pharmacological substances affect the nervous system.

The second method² is that employed by the present writer in conjunction with G. J. Burch. This consists in a special adaptation of the plan of research used by Burdon Sanderson in studying the electromotive phenomena of muscle.

It will be sufficient here to mention that it is dependent upon the employment of a special instrument, the capillary electrometer. This is particularly sensitive to rapid electrical change, and indicates the presence of such changes by a movement in the surface or meniscus of a fine mercurial column. The movement can be recorded by project-

¹ Waller: "Observations upon Isolated Nerve".—*Phil. Trans.*, 1896. Croonian Lecture.

² Gotch and Burch: "The Electrical Response of Nerve to a Single Stimulus investigated with the Capillary Electrometer".—*Proc. Royal Society, London*, cxiii., p. 300, 1898.

ing the column by means of a microscope upon a travelling photographic plate. In nerve the difficulty lies in the circumstance that the movement produced by nerve electrical changes must under all conditions be extremely small in extent, and that in addition to this it is of very short duration. It is thus impossible to follow it with the eye even when the magnified image of the mercurial column is so enlarged by projection as to be 400 times the size of the object. If, however, the plate is travelling with sufficient velocity the moving shadow thrown by the enlarged image, although too transient to be perceptible to vision, is found to have occurred when the plate is developed. In this way records have been obtained of the electrical disturbance evolved in nerve by each single stimulus, and convincing proof is afforded that this single electrical response, starting from the seat of stimulation, is successively assumed by each more distant portion of a continuous nerve tract. The method has the distinction of being the only one by which up to the present the single change has been recorded. A large number of experiments have been made with nerves under different conditions, and the modifications in the response consequent upon these have been recorded. We have here the start of an inquiry which will, it is hoped, effect for nerve what the graphic record has done for muscle, since each record is a graphic account of the nerve change as given by such an historian as the electrometer. Is the electrometer a reliable historian or has it a large personal bias? This question admits of much discussion, and has been taken up adversely by several of the leading German physiologists, particularly Hermann.

Without entering into the somewhat technical controversy, this much may be stated here. Each electrometer has its own rate of movement, its own personal equation; every record has therefore to be interpreted afresh after making due allowance for this personal bias of the instrument. Fortunately the amount and character of the bias remains approximately constant for any one instrument, so that if it can be determined the allowance is easily made for all records obtained from the particular instrument whose

characteristics are thus known. It has further been shown by Burch that the allowance having been made by taking appropriate measurements, the true history of the electrical disturbance may be always deduced from the distorted account recorded in the curve of the electrometer displacement, since the degree and character of the distortion are constant. This has now been done in the case of the records obtained from nerve, and if correct the deductions thus obtained indicate for the first time the magnitude, duration and character of the excitatory electrical disturbance of nerve. The present writer has a firm belief that before long similar methods of investigation will be more extensively applied to the study of nerve by those who up to the present have chiefly utilised older methods; if this should take place he feels convinced that as fruitful a field of research lies before those who are studying nerve phenomena as lay before physiologists when Helmholtz introduced the graphic method in connection with the study of muscle.

FRANCIS GOTCH.

NOTES ON PARASITES.

II.

THE parasitic habit of life is usually associated with certain structural peculiarities which present themselves in a greater or less degree in all the Entozoa. Among such peculiarities the most striking are (1) the presence of hooks and suckers or other objects of adhesion ; (2) the shape of the body, adapted for insinuating itself into various spaces of the unfortunate host or for boring into his tissues, and in those parasites which live in such passages as the alimentary canal, being so moulded as to offer little resistance to the passage of food so that the intruders are not readily dislodged ; (3) the tendency of the sense organs and nervous system to diminish ; (4) the corresponding increase in complexity both in the structure of the reproductive organs and in the details of the development of their products ; and finally (5) the tendency to do without a functional alimentary canal.

There are perhaps two other structures in the bodies of the larger Entozoa which show profound modifications possibly connected with their endo-parasitic mode of life. These are the epidermis and the body-cavity. In a former number of this journal,¹ I have described the condition of the skin in Cestodes and Trematodes as interpreted by Prof. Blockmann of Rostock. The condition of the epidermis in Nematodes and in the Acanthocephala is even more remarkable. Within the clearly defined cuticle which covers the bodies of these animals, is a layer of protoplasm more or less differentiated into minute fibrils and scattered through the substance of this layer are a certain number of nuclei. No cell limits are to be seen although they exist in the early stages of the development of the embryo.

This peculiar tissue is in the Nematodes heaped up into four ridges which run down the body, one dorsal, one

¹ SCIENCE PROGRESS, new series, vol. i., p. 78.

ventral and one on each side. These ridges divide the circular outline of a section through the body of a Round Worm into equal quadrants. Each ridge pushes between the muscle-cells which line the inner surface of the rest of the epidermal tissue and freely projects into the body cavity (Fig. II. *b*). The dorsal and ventral ridges surround the longitudinal nerves, the lateral ridges embrace the excretory canals.

There seems no reason to doubt that these ridges are epidermal in structure. On each side of them lie the mesoblastic musculo-epithelial cells, so characteristic of the Nematodes (Fig. II. *c*), which, together with the above-mentioned ridges, bound a spacious cavity—the so-called body-cavity—on the outside. This cavity is traversed by the alimentary canal, which for the greater part of its length consists of a single layer of cells, there being apparently, in these animals no mesoblastic perivisceral layer such as is ordinarily found in the Triploblastica or animals with the three well developed embryonic layers.

Thus in the Nematoda the body-cavity has very peculiar and, in fact, unique relations. On the outer side it is bounded for the most part by mesoblast, but at the four axes by epiblast, and on the inner side it is limited by hypoblast. The relationship of this space to the embryonic layers is closely paralleled by that of the blood-containing spaces of such Cœlomata as the Vertebrata, which many years ago were shown, both on theoretical grounds¹ and by actual observation on the developing Lamprey,² to be primarily spaces between the epiblast and the hypoblast, or in other words, to be part of the segmentation cavity. This space in the higher forms subsequently becomes surrounded by mesoblast and forms the cavity of the blood vascular system.

Thus it is obvious that the space between the body-wall and the alimentary canal of Nematodes is not homologous with the cœlom of the Chætopoda, Mollusca, Arthropoda, Vertebrata, etc. The fact that it is not is further supported

¹ Bütschli, *Morphol. Jahrb.*, Bd. viii., 1883, p. 474.

² Shipley, *Quart. Jour. Micr. Sci.*, vol. xxvii., 1887, p. 325.

by the relations which it bears to the excretory system and to the reproductive organs. The former are not formed from its walls, and with the exception of certain aberrant species,¹ *Lecanocephalus* and *Dochmius*, the latter do not open into its lumen.

This space contains a corpusculated fluid, and probably it acts to some extent both as a blood-space and as a lymph-space. Within the last year a new light has been thrown on what goes on in the body-cavity of these lowly parasites by Prof. Nasonov² of Warsaw, who, utilising the methods of Kowalewsky, has demonstrated the existence in Nema-

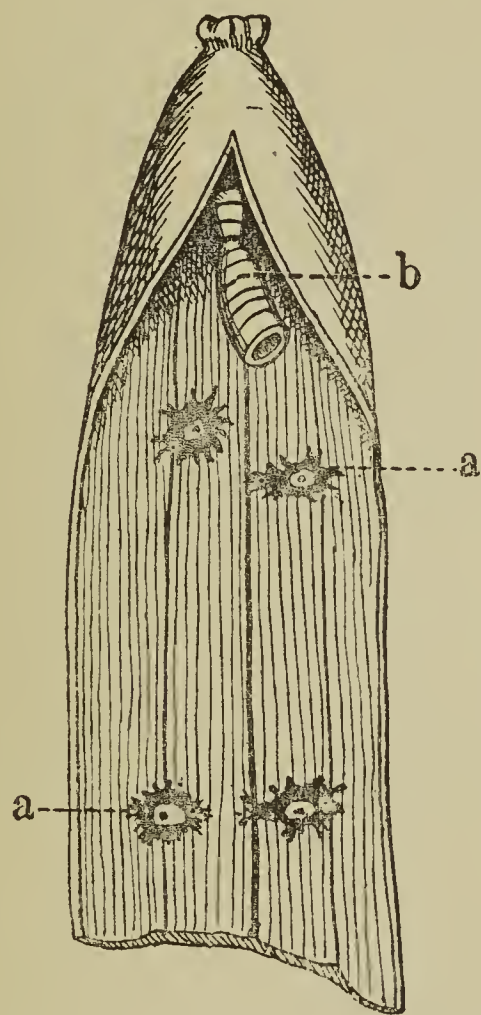


FIG. 1.—Anterior extremity of the body of *Ascaris megalocephala*. x. 1·2. From Nasonov. The interior is exposed by a dorsal incision, two hours after an injection of Indian ink.

a. The star-like bodies in which the ink particles accumulate. The clear space is the nucleus of the giant cell.

b. The alimentary canal.

todes of certain gigantic cells which support others whose function it is to dispose of the foreign bodies which may, from some reason or other, make their way into the body-cavity of these creatures.

¹ Hamann, *Sb. Ab.*, Berlin, 1891, p. 57.

² *Zool. Anz.*, Bd. xx., 1897, p. 202, and *Arch. Parasit.* T. I., 1898, p. 170.

There has been the usual controversy as to who first saw these giant cells, but this much-debated question is comparatively unimportant, and no one, I think, denies that the Polish professor first demonstrated their function. By injecting carmine or Indian ink into the bodies of *Ascaris megalocephala* and *A. lumbricoides*, waiting some hours

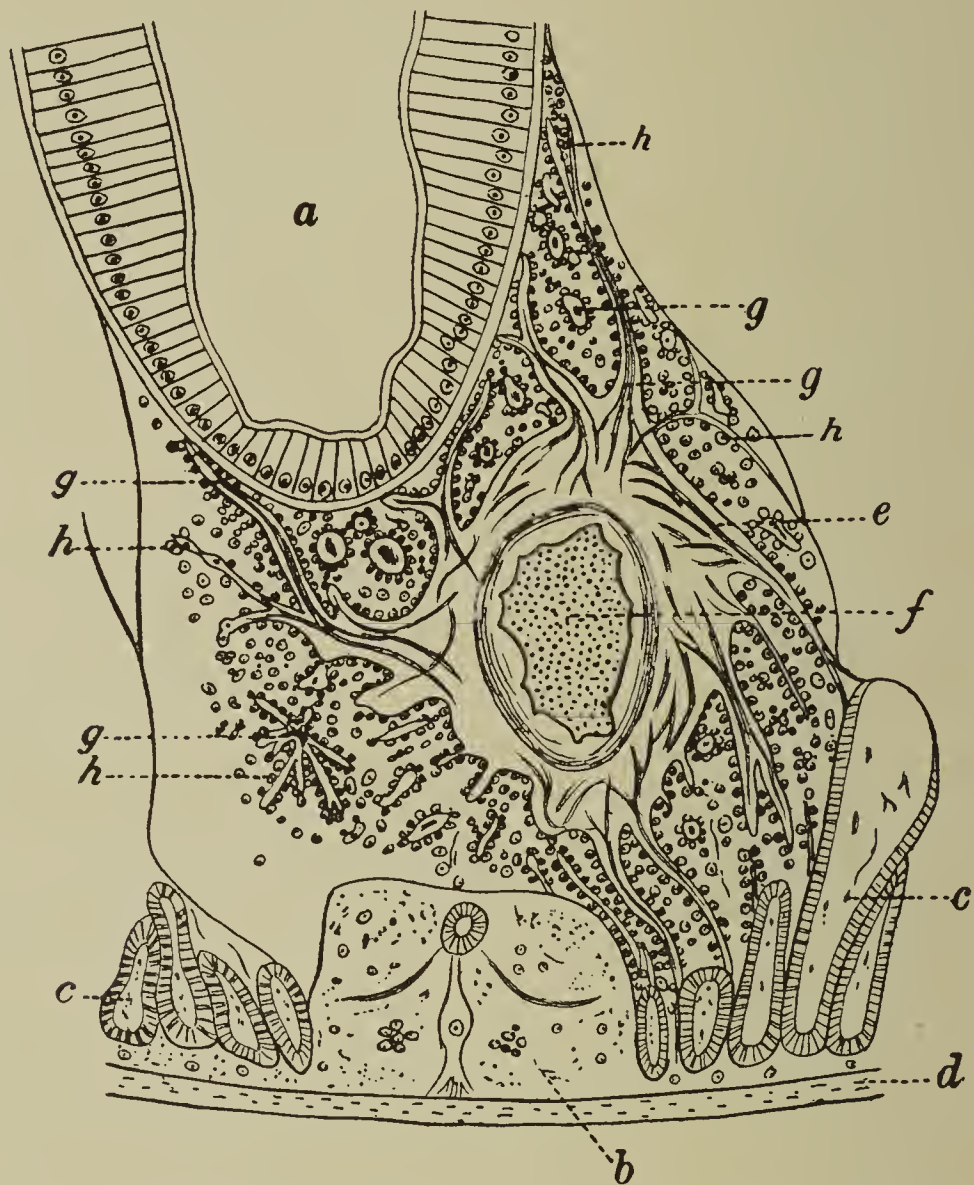


FIG. 2.—Transverse section through a star-shaped body, part of the alimentary canal and the skin of an *Ascaris megalocephala*. x. 85. From Nasonov.

- a. The intestines.
- b. The ridge of epidermis which surrounds the excretory vessel.
- c. The muscle-cell.
- d. The cuticle.
- e. The giant cell.
- f. The nucleus.
- g. The fibrils of the giant cell.
- h. The minute cellules which surrounds it.

and then investigating the parasites, he found that four enormous cells with branching processes, almost invisible under ordinary circumstances, had become so stained with the foreign material as to be visible even through the skin of the animal. (Fig. I. a.)

These four cells lie on the same level as the lateral excretory canals, two anterior and two posterior, but all lie in the anterior third of the animal. The cells are very large, in *A. megalocephala* attaining a length of 1 cm. and in *A. lumbricoides* of 3 mm. Each contains an enormous nucleus—so large as to be visible to the naked eye—and each breaks up peripherally into a meshwork of fibrils which attach themselves to the lateral lines, to the exterior of the alimentary canal and to the muscle-cells of the body wall. (Fig. II. *g.*)

The branching fibrils which run from the body of the cell to the neighbouring organs are coated with numerous spherical or pyriform bodies (Figs. II. *h.* and III. *b.*) in each of which Nasonow recognises a nucleus, in fact he regards them as minute cells attached not only to the fibrils but to the body of the giant cell. When carmine or Indian ink is



FIG. 3.—The end of two fibrils of the giant cell. From Nasonov.

a. The fibril.

b. The phagocytic cells.

injected into the body of the Nematode, the granules are taken up by these minute cells and accumulating around the fibrils and body of the giant cell render it visible even to the naked eye. In the same way they take up the bacilli of Anthrax or Tubercle bacilli and within their body these foreign germs are disintegrated and destroyed. Injection of appropriate reagents demonstrate that the reaction of these cells is an acid one. When kept at a temperature which does not materially differ from that of their host, and the giant cell is observed alive, it is seen that the minute cellules which surround it are capable of amœboid movements which no doubt assist in the ingestion of foreign particles.

We have, then, in the body-cavity of Nematodes at two different levels, a sieve consisting of two giant cells with processes spreading across the lumen of the space, and

each process clothed with minute cells eager to eat up any foreign matter which comes in its way, the whole forming a simple but apparently very efficient lymphatic gland. Whether these cells have a more intimate relation with the excretory canals on which they are situated, and whether the minute cellules which clothe them are not the corpuscles of the perivisceral fluid, temporarily come to rest, are at present speculations which it is to be hoped further investigation will clear up.

The last Numbers of the Studies from the Zoological Laboratory of Nebraska (Nos. 24 and 26) published under the direction of the eminent helminthologist, Henry B. Ward, contains an interesting summary of the number of parasites found in a given number of cats and dogs examined in the town of Lincoln, Nebr. Two of the parasites of the cat *Dipylidium caninum*—also found in the dog—and a Distomum, *Distomum felinum*, have been found in man. The latter is not uncommon in the peasants of Siberia, the former is, however, rare, and is as a rule confined to children of immature years. There is reason to believe that the second host of *D. caninum* is the *Trichodectes canis*, and children possibly become infected with it whilst playing with dogs.

Ward's investigations on the cat are not so extensive as those on the dog, and are less interesting because similar investigations have not been carried on elsewhere, consequently no data exist for a comparison between the number of parasites infesting the cats of the ancient cities of the Old World with those of a comparatively new town like Lincoln.

The following table compiled by Deffke, with the addition of the results of Sommer's investigations in Washington, D.C., and of Ward's at Lincoln, give some idea of the parasites of the dog in various parts of the world. The list does not include the Trematode *Hemistomum alatum*, recorded by Schoene in Saxony and elsewhere, nor does it include insect larvæ, Pentastomas or a species of *Echinorhynchus* found by Ward at Lincoln, in fact it is confined to the two orders, Cestoda and Nematoda.

Examiner and Locality.	<i>Tænia serrata.</i>	<i>Tænia marginata.</i>	<i>Tænia cœnurus.</i>	<i>Tænia serialis.</i>	<i>Tænia echinococcus.</i>	<i>Dipylidium caninum.</i>	<i>Bothriocephalus latus.</i>	<i>Bothriocephalus fuscus.</i>	<i>Cysticercus cellulosæ.</i>	<i>Echinococcus polymorphus.</i>	<i>Mesocystoides lineatus.</i>	<i>Ascaris mystax.</i>	<i>Uncinaria trigonocephala.</i>	<i>Spiroptera sanguinolenta.</i>	<i>Eustrongylus gigas.</i>	<i>Trichocephalus depressusculus.</i>	No. of animals examined.	Percentage infected.
Krabbe, Copenhagen . . .	—	17·3	2·16	—	1·08	47·03	—	—	—	—	—	20·54	1·62	—	—	—	—	65
Krabbe, Denmark . . .	0·2	14	1	—	0·40	48	0·2	—	—	—	—	24	2	—	—	—	500	72
Krabbe, Iceland . . .	—	75	18	—	28	57	—	5	—	—	21	2	—	—	—	—	100	100
Thomas, Victoria . . .	—	40	—	—	40	60	—	—	—	—	—	27	—	—	—	—	—	90
Thomas, South Australia . . .	—	27	—	—	40	60	—	—	—	—	—	27	—	—	—	—	—	80
Schoene, Leipzig or Saxony . . .	15	27	1	—	—	25	—	—	1	1	—	24	1	—	—	—	100	93
Deffke, Berlin, Germany . . .	—	57	0·5	—	1	40	0·5	—	—	—	—	18·5	4·5	2	1	—	200	62
Sommer, Washington, D. C. . .	12	2	—	—	—	44	—	—	—	—	—	28	56	2	2	70	50	96
Ward, Lincoln, Neb. . . .	4·5	5	—	5	—	65	—	—	—	—	—	20	10	—	—	—	20	75

With regard to the members of the two orders mentioned in the table, certain points in their geographical distribution are of interest. *Tænia marginata*, the largest *Tænia* which inhabits the dog, *Dipylidium caninum* and *Ascaris mystax* occur in all the widely distributed places mentioned. *T. serrata* is absent from Iceland, that home of dog-parasites, where 100 per cent. of the dogs are infested with some form or other of Entozoa. *T. cænurus*, whose larval form infests the brains and more rarely the spinal marrow of sheep, is at present unknown in America, and is not mentioned in Thomas' lists from S. Australia and Victoria. The loss annually caused by the "Gid" or "Staggers," as the disease caused by its presence is called, has been estimated at 1,000,000 sheep in France and 35 per cent. of all the flocks in England, if the season be a bad one. Naturally it is commonest in sheep-dogs and comparatively rare in town-bred animals. *T. serialis* is also rare, in fact in some recent lists the name of its host is preceded by a query. It, however, undoubtedly occurs in the dog though only recorded on our list from Lincoln. *D. caninum*, as mentioned above, occurs both in the cat and the dog.

The really important parasite, from a merely human point of view, is the *Echinococcus polymorphus*, "the most insidious and dangerous parasite which inhabits the human system". In Iceland and in Australia it infests about 30 per cent. of the dogs examined. And when we are reminded that in the former island there are about twenty-eight dogs to every hundred inhabitants, and that during the long winter days men and beasts live together in the same huts without sanitary precautions, it is not surprising to learn that one in every forty-three individuals suffers from *echinococcosis*. Happily this form is absent from America, though Sommer has recorded 100 cases of the disease from the medical literature of the last fifty years. Probably the sufferers brought the parasite with them into the new country.

The result of the Nebraska research shows that on the whole there is a large percentage of harmless parasites in the dogs of that State and an excessive rarity—speaking

again from a purely human standpoint—of dangerous forms. These facts are to some extent explained by the recent settlement of the country and the uncontaminated condition of the fields. This state of affairs, coupled with the existence of large slaughter-houses where, as the meat-packers eloquently express it, “The only part of the hog which goes to waste is the squeal,” is to a great extent responsible for the absence of those parasites which afflict the human species whether through his person or his pocket.

Of the Nematodes enumerated in the table, *Ascaris mystax* is rarely found in man, its accidental introduction into the system being due probably to the fondling of dogs or cats. *Uncinaria trigonocephala* produces a profound anæmia in dogs, especially in the sporting species. It occurs also in cats and gives rise to the disease known in Italy as *tifo dei gatti*, but does not attack man. The other species mentioned in the list are comparatively unimportant.

To the student of parasites one of the most important features of the present year is the appearance of the *Archives de Parasitologie*, under the able editorship of Prof. Raphael Blanchard, of the Faculty of Medicine, Paris. However much the multiplication of new journals is to be deplored, there was undoubtedly room for a periodical devoted exclusively to the study of parasites, whether they affect man or other animals, and judging from the first three numbers, the *Archives* are worthy to fill the vacant place.

Two features about the journal are new. One is the appearance of Spanish in addition to the four languages, French, English, German and Italian, to which polyglot zoological publications have hitherto been restricted. There is much useful work on Helminthology and Bacteriology being done especially in the Spanish-speaking Republics of South America—the opportunities are great—which only finds expression in the Spanish tongue; the cost of learning enough of the language to follow a scientific treatise is small, so that on all grounds we applaud the courage of Prof. Blanchard in adding Spanish to the list of languages in which articles for the *Archives* may appear.

The second feature which seems to us even more commendable is the appearance of a series of articles dealing with the history of renowned Helminthologists. The editor himself contributed to the April number a sketch of the life and work of Prof. R. Leuckart, illustrated by a characteristic portrait and a facsimile letter, and in the current issue is an admirable account of Francesco Redi. This remarkable physician, poet, courtier, lexicographer and zoologist,¹ who was born at Arezzo in 1626 and was buried in the church of San-Francesco in the same Tuscan city in 1697, was one of the earliest and greatest investigators into the anatomy and habits of Entozoa. He investigated with that object not only the bodies of numerous Vertebrates but also those of Cephalopods and Crustacea, and was the first to describe the Liver-fluke, a fact recorded by the name *Redia*, given to the larval Trematode by Filippi who first discovered it. Altogether Dr. Guiart records some seventy-five species of Entozoa studied by Redi.

The articles of a more strictly scientific nature are too numerous to mention; besides the more lengthy memoirs there are numerous short notices and reviews, so that the whole forms a compendious survey of the advance made day by day in the study of Parasitology.

ARTHUR E. SHIPLEY.

¹ If we may trust the poet, Redi was among the small number of teetotalers who have sung the praises of wine.

“ Even Redi when he chaunted
Bacchus in the Tuscan valleys,
Never drank the wine he vaunted
In his dithyrambic sallies.”

—*Drinking Song*. H. W. LONGFELLOW.

FLORAS OF THE PAST.

WEALDEN.

BY far the best known of the ancient floras are those of Upper Carboniferous and Lower Permian age. The wealth of material afforded by the sandstones, shales and coal-seams of the Upper Carboniferous system, and more especially by the petrified plant fragments from the calcareous nodules of the Lancashire and Yorkshire coal-fields, and the siliceous nodules of French and German localities, have enabled palæobotanists to reproduce a fairly complete picture of the vegetation of the Coal period. The excellent state of preservation of many of the older fossil plants has compelled the attention of botanists whose chief concern is with the plants of to-day. In addition to the splendid results obtained from a detailed anatomical investigation into the structure of special types of Coal-Measure and Permian plants, there are other facts of considerable interest to be gained from a general revision of Palæozoic floras from a phytogeographical point of view. Interesting results have already been obtained in this field of work,¹ but a critical comparative study of the component elements of the various Palæozoic floras should lead to the discovery of valuable evidence bearing on the questions of plant distribution and the existence of botanical provinces towards the close of the Palæozoic epoch. It is the botany of the oldest and, on the other hand, of the most recent geological periods that has so far received the chief share of attention at the hands of botanists, while the floras of the Mesozoic epoch have not hitherto attracted much notice from others than specialists.

In the present article it is proposed to attempt a brief summary of the general features of the vegetation of the Wealden period, special reference being made to facts which may prove of interest to botanical readers who are

¹ Zeiller. [*Vide* also SCIENCE PROGRESS (new series), vol. i., No. 2, p. 178, 1897.]

not familiar with the fossil botany of post-Carboniferous rocks. Although on the whole the plants from Mesozoic horizons occur in the form of casts and impressions, and the number of well-preserved petrifications is extremely small in comparison with the material supplied by the older rocks, yet there are many facts of considerable botanical interest to be gleaned from an examination of Jurassic and Cretaceous floras. There is perhaps a tendency on the part of those whose main work is confined to recent plants, but who occasionally refer to palæobotanical literature with a view to giving a greater completeness to their conclusions, to confine themselves to the botanical evidence afforded by Palæozoic plants. For this and other reasons it is proposed to give a short *résumé* of such data as we possess as to the composition of some of the floras of more recent geological age.

The geological system to which the name Wealden was first applied in 1828 has usually been placed by geologists at the base of the Cretaceous formation. It would seem, however, that some at least of the strata comprised under the term Wealden are more closely linked by the animal and plant fossils which they contain with the Jurassic rather than with the overlying Cretaceous system. In England the Wealden rocks are chiefly developed in the Weald district of Kent and Sussex; they occupy an oval-shaped area, bounded by the chalk escarpment, which extends from Folkestone Hill through the counties of Kent, Surrey, Hants and Sussex, to the sea at Beachy Head. Strata of the same age are also exposed on the south-west coast of the Isle of Wight, between Compton Bay and Atherfield Point, and to a less extent in the neighbourhood of Sandown, on the opposite coast. Since the early part of the century it has been recognised that the Wealden deposits are of a freshwater and fluviatile nature, and were evidently laid down in the delta of a large river which flowed through a low-lying country supporting a luxuriant vegetation. Fragmentary relics of the English Wealden flora were first described about seventy years ago by Mantell, Fitton, Brongniart and others, but it is only in recent years that

the rich collections which are now in the British Museum have been discovered; and considerable credit is due to Mr. Rufford of Hastings for having been the means of obtaining an unusually fine series of plants from the Wealden beds on the Sussex coast.

In the Island of Portland and on the neighbouring mainland coast near Swanage and Lulworth there is exposed a succession of strata, for the most part of freshwater origin, but containing a few intercalations of marine deposits. This series—long known as the Purbeck beds—is closely connected with the succeeding Wealden strata, both as regards fossil contents and conditions of deposition; it forms a part of the delta and lagoon deposits which were laid down in Southern England at the close of the Jurassic period.

The Wealden plants occur for the most part as fragmentary portions of twigs and leaves which were floated into their present position by the quiet waters of a river which rivalled in size some of the largest streams of to-day. In the Purbeck beds, and in the Wealden of Brook Point in the Isle of Wight, we have huge pieces of coniferous wood and cycadean stems which grew where their remains are now found as fossils, or were floated a short distance from the neighbouring forests.

From France but few Wealden plants have been recorded. In Northern Germany, on the other hand, there is an extensive development of freshwater Wealden beds, which has supplied Schenk¹ and other continental writers with rich material for palæobotanical study. Plants of the same age have been found also to the north of the Hartz Mountains and elsewhere. From Austria, Portugal,² Russia and the arctic regions³ Wealden plants have also been obtained. The famous Belgian locality of Bernissart, which has furnished the Brussels Museum with more than twenty skeletons of *Iguanodon*, has afforded a few samples of Wealden vegetation.

There are a few other localities in Belgium—notably Bracquegnies—where plants have been found, but the

¹ Schenk (1) and (2).

² Heer (1) and Saporta.

³ Heer (2), Nathorst (2).

Bernissart beds are of special interest from the fact that by far the greater number of plant fragments belong to ferns, while cycads and conifers are barely represented. The Wealden deposits, which fill up a deep cañon cut through the Carboniferous rocks at Bernissart, appear to be the old sandy muds of a river which flowed through a marshy region inhabited by the ungainly Iguanodons and carpeted with a dense growth of ferns.¹ Some of the most important Wealden districts are in the North American continent, and the rich flora which they have afforded has recently received exhaustive treatment at the hands of Prof. Lester Ward. From Canadian rocks Sir William Dawson² has also recorded species of Wealden plants, but it is in the United States that the flora of this period is best represented. In 1889 Prof. Fontaine³ published one of the United States Geological Survey monographs on the "Flora of the Potomac Series," and in this work there are described numerous well-known European species of Wealden plants. It has since been shown⁴ that the Potomac formation—so named by McGee in 1885—includes rocks ranging in age from Jurassic to well up in the Cretaceous system; some of the lower plant-bearing strata are undoubtedly of the same geological age as the Wealden of Europe, and these have furnished many interesting additions to our list of Wealden species. Good evidence has recently been published of the existence of a Wealden vegetation in Japan,⁵ and from Africa and New Zealand Wealden species have also been recorded.⁶

We will first pass in review some of the best-defined types of Wealden vegetation, and afterwards summarise the chief features of botanical interest, neglecting such plants as cannot be referred with certainty to a definite systematic position.

ALGÆ.—The majority of the so-called algæ from Wealden rocks are of no botanical importance, and at best merely afford

¹ Dupont.

² Dawson.

³ Fontaine.

⁴ Ward (3).

⁵ Yokoyama, Nathorst (1).

⁶ For reference to the existence of Wealden species in these countries, *vide* Seward (1) and (2).

evidence of the existence of algal species very similar in the form of the thallus to certain recent seaweeds. We have on the other hand some good examples of the genus *Chara* which demonstrate the abundance of plants practically identical with our modern freshwater stoneworts.

BRYOPHYTA.—Under this head there are no Wealden species of special interest. Such fossils as may reasonably be referred to this class have a close resemblance to *Marchantia* as regards the vegetative form of the plant.

PTERIDOPHYTA.—The two divisions of the Pteridophyta most abundantly represented are the Ferns and Horsetails. We will briefly consider the latter first and then pass on to a few representatives of ferns.

1. *Equisetaceæ*. The numerous stem-casts of various species of *Equisetites* prove the abundance of slender Horsetails in the Wealden vegetation. None of the stems are much thicker than those of the larger species still living in Europe, and so far as we know the fossil forms agreed with their present-day descendants in the absence of secondary wood. The Equisetums of the Lower Oolite, and more especially those from Triassic rocks, had much thicker stems, and very probably grew in thickness in a manner comparable to the growth of the still older Calamites. The rhizomes of some of the Wealden Equisetums bore branches with tuberous internodes exactly like the oval tubers of *Equisetum arvense* L., *E. maximum* Lam. and other species.

2. *Filicinæ*. Unfortunately many of the Wealden ferns have as yet afforded little or no trustworthy evidence as to their systematic position, and in many cases we cannot do more than class the sterile fronds under provisional generic names, or at best hazard a guess as to the family to which they belong. There are however certain exceptions in which the family characters have been satisfactorily determined. The literature on Jurassic and Lower Cretaceous floras contains the names of several recent genera of ferns which are supposed to be represented by fossil species, but in not a few instances the choice of the name of a recent genus has been made without any real evidence to support it. Some of the most misleading determinations in

palæobotanical writings are those relating to fern fronds ; a mere external resemblance in form between an imperfect specimen of a fossil frond and the leaf of a recent species has often proved too strong a temptation for even an experienced specialist. Many of the Mesozoic ferns referred to *Thyrsopteris*—a genus represented to-day by a solitary species confined to Juan Fernandez—*Aspidium*, *Hymenophyllum*, *Dicksonia* and other genera have no claim whatever to be accepted as well-established representatives of these generic types.

One of the commonest and most characteristic Wealden ferns is that named by Brongniart in 1828 *Sphenopteris Mantelli* ; the tripinnate fronds of this plant are ovate-lanceolate in form, with narrow uni-nerved ultimate segments. The fertile pinnæ bear ovate and somewhat swollen segments, which present a striking agreement with those of some species of the recent genus *Onychium*. Although no individual sporangia have been preserved, the form of the fertile segments—in addition to the general similarity of habit—points to a close affinity with some of the tropical species of *Onychium*. On the ground of this resemblance, the generic name *Onychiopsis* has been substituted for that of *Sphenopteris*.

Casts of a Tree-fern stem usually referred to the genus *Protopteris*, but spoken of by some writers as *Dicksonia*, are not uncommon in rocks of Lower Cretaceous age ; the stem is covered with crowded spirally disposed petiole-bases, in each of which the form of a horseshoe-shaped leaf-trace is clearly shown, and in an English example the vascular tissues of the stem have been partially petrified. In form and structure this genus presents a striking agreement with the recent Tree-fern *Dicksonia antarctica* Lab., and it is highly probable that this species is closely related to the Wealden plant. Some fronds of Wealden age have also been referred to *Dicksonia*, and these lend a certain amount of support to the comparison which has been made of the *Protopteris* stems with those of the recent genus.

A Wealden fern originally described by Dunker¹ from

¹ Dunker, p. 5, pl. ii., fig. 2.

North Germany as *Pecopteris Althausii*, and afterwards re-named by Schenk¹ *Matonidium Göpperti*, is closely allied to the living Malayan genus *Matonia*. Specimens of *Matonidium Göpperti* have been found in Wealden strata in Germany, Portugal, England and elsewhere ; in the flabellate habit of the frond, and in the form of the sori, these fossils exhibit a striking likeness to *Matonia pectinata* Brown. The two living species of *Matonia*, *M. pectinata*, first figured by Wallich, and diagnosed by Robert Brown in 1830,² and *M. sarmentosa*,³ described by Baker in 1888, are confined to Borneo and the Malay Peninsula ; the genus has long been recognised as a type apart among present-day ferns, and some authors have placed it in a tribe by itself—the Matonineæ. Among Mesozoic ferns we have the Wealden species *Matonidium Göpperti* (Ett.), *M. Wiesneri* Krasser, recently described⁴ from a somewhat higher horizon, and other species of the same genus ; with *Laccopteris*, a Rhætic genus, and *Microdictyon Dunkeri*, another Wealden fern, which most probably belong to the same tribe as the recent *Matonia*.

An exceedingly interesting fern has recently been found by Dr. Bommer, of Brussels, in some sands of Wealden age near Bracquegnies ; the plants from this locality have not yet been described, and I am indebted to Dr. Bommer for showing me the rich material on which he is preparing a memoir. The plant fragments were found in a loose sandy matrix ; they occur as dark-brown twigs or leaves, and suggest the dried and discoloured pieces of recent plants which have been covered up for some years in a bed of sand. The stems and leaf-stalks are not crushed, and occasionally pieces of pinnæ with thin black pinnules still intact may be picked out of the Wealden sands. Among the ferns, the most interesting specimens are those of a *Gleichenia* ; the habit of the fronds, the form of the pinnæ and pinnules, and the shape of the vascular strands, demonstrate beyond a doubt the generic affinities of the fossil.

¹ Schenk (1) p. 219, pls. xxvii., xxviii. and xxx. ; (2) p. 160, pl. xxvii.

² Wallich. ³ Baker (*vide also Linn. Journ.*, vol. xxiv., p. 256).

⁴ Krasser, p. 119, pls. xi., xii. and xvii.

Among Wealden ferns, which may be referred to as probable representatives of definite families or genera, the following may be mentioned :—

Ruffordia Göpperti (Dunk.)¹ (? Schizaceæ), species of *Acrostichopteris*² (cf. *Acrostichum*), also species referred to *Adiantum*,³ *Aspidium*² and other genera.

GYMNOSPERMÆ.—I. *Cycadales*. The abundance of pinnate fronds of different species of “Cycads” is a characteristic feature of both Wealden and Jurassic rocks. In endeavouring to determine fossil cycadean leaves, we have to bear in mind the fact that it is practically impossible in many cases to be certain whether the fronds were borne by the stems of true Cycads or by stems of the genus *Bennettites*, which belongs to another division of the Cycadales. The Bennettiteæ represent an extinct group of plants which flourished during the Jurassic and Lower Cretaceous epochs ; in certain respects they agree very closely with the recent Cycadeæ, which are represented by *Cycas*, *Zamia*, *Dioon*, *Encephalartos*, and a few other genera practically confined to tropical regions. The stems of Cycads—or “Sago Palms,” as they are sometimes popularly called—and of the Bennettiteæ resemble one another in general appearance, but the nature of the reproductive organs in the latter family precludes their inclusion among the true Cycads. In the Cycads the male and female flowers are borne at the apex of the trunk, and with the exception of the female flowers of the genus *Cycas*, they have the form of cones consisting of crowded sporophylls ; in the Bennettiteæ, on the other hand, there are special branches given off at right angles to the main stem, which terminate in an inflorescence of a more complex and highly differentiated type than the flowers of the Cycads. The existence of these fertile lateral shoots forms a ready means of distinguishing a Bennettitean from a Cycadean stem ; the stem of *Bennettites* is enclosed in an “armour” of persistent leaf-bases as in the ordinary Cycad, but there are crowded series of smaller scale-leaves here and there in the axil of a petiole, which mark the position

¹ Seward (1), p. 76 ; pls. iv.-vi.

² Fontaine, pls. clxxi. and clxxii.

³ Saporta, pls. x., xviii., xxix., etc.

of lateral shoots bearing linear lanceolate scale-leaves. These shoots terminated, at least in some species, in an inflorescence of the type to which Carruthers gave the name *Williamsonia*. Prof. Lester Ward¹ has suggested the inclusion of all fossil cycadean stems in the sub-family Cycadeoideæ of Robert Brown. Seeing how distinct a type the genus *Bennettites* represents, it is, I believe, much better to retain the family Bennettiteæ, and to use the generic name *Cycadeoidea* of Buckland for certain fossil stems which cannot be certainly referred to *Bennettites*.² In strata of Wealden and Lower Greensand age stems of the *Bennettites* type are fairly common; by far the most numerous are the large silicified stems from the Black Hills of Dakota, from Maryland and other districts of North America.³ The English species *Bennettites Gibsoni* Carr, from Lower Greensand rocks, is the most perfectly preserved specimen so far described; other English species occur in the Purbeck beds of Portland and elsewhere. From Italy, too, several examples have been described by Counts Capellini and Solms-Laubach.

There are certain species of cycadean stems from Wealden and Purbeck beds which agree with the recent Cycads in having no lateral fertile shoots, and which may probably have borne terminal flowers; traces also of cycadean flowers have been recorded. It would seem, then, that both true Cycads and the extinct Bennettiteæ were in existence side by side in the Wealden period. Unfortunately the cycadean fronds, such as species of *Otozamites*, *Dioonites*, *Zamites*, etc., which are common Wealden fossils, are found in the rocks apart from their stems, so that we must speak of them as fronds of "cycadean" plants in the wide sense, remembering that probably many of them were borne by stems of the Bennettiteæ.

2. *Coniferæ*. Among the numerous examples of coniferous twigs, wood and leaves there are several species which may be safely referred to definite genera of existing

¹ Ward (5), p. 5.

² Seward (4).

³ A splendid example of an American cycadean stem has recently been added to the Fossil Plant Gallery of the British Museum.

conifers. The well-known "Pine-raft" at Brook Point in the Isle of Wight and the Purbeck beds of Portland have furnished numerous specimens of coniferous wood, with the structural details more or less perfectly preserved. Prof. Lester Ward has recently given some account of the Wealden and Purbeck woods from English localities, and states that all the fossil wood is of the Araucarian type.¹ This must refer, however, to such wood as Prof. Ward has himself examined; it is certainly not a statement which can be accepted as covering all the known woods from the Wealden-Purbeck strata. It is a matter of regret that no one has so far made a careful examination of the silicified woods from the Purbeck beds of Portland, nor to any great extent of those from the Isle of Wight; Prof. Ward justly remarks: "It is surprising that no one in England has thought to describe or name these fossil woods, and I would not have ventured to do this on the imperfect material in my possession, if it had not seemed to be the only way in which they could be brought into their systematic position as an integral part of the fossil flora of the Wealden".² Two specimens of wood, one from Portland and the other from the Isle of Wight, are described by Dr. Knowlton in Prof. Ward's memoir; both are referred to as *Araucarioxylon*, but the preservation is far from satisfactory, as admitted by Dr. Knowlton, and demonstrated by his figures.³ Visitors to Portland are probably familiar with a silicified stem, about 20 feet long, fixed to the front of a house in Fortune's Well Street; this stem was briefly referred to by Fitton in 1834, and the same stem has now been named by Lester Ward *Araucarioxylon antediluvianum*,⁴ the specific name being chosen from that of the house against which the stem is placed. It is to be regretted that the name *Araucarioxylon* has been adopted without the evidence of internal structure; no one, so far as I am aware, has ever seen sections of this particular stem. Prof. Ward has done good service in reminding us of a strangely neglected duty, but the determination of the Purbeck and Wealden woods must be the

¹ Ward (4), p. 491.

² *Ibid.* (4), p. 496.

³ *Ibid.*, pp. 495, 496, pl. cii.

⁴ *Ibid.*, p. 491.

result of careful and systematic investigation of well-preserved specimens, or the names assigned will possess no scientific value. An exceedingly careful piece of work of this kind has just been completed by Mr. C. A. Barber,¹ who has investigated the structure of some coniferous wood of Lower Greensand age from the Isle of Wight; he names the species *Cupressinoxylon vectense* Barb. Dr. Knowlton² has also described a species of the same genus from rocks of approximately Wealden age near Washington.

The genus *Pinites* is well represented not only by petrified wood possessing the anatomical characters³ of the recent *Pinus*, but also by numerous leaves, twigs and cones. The genus *Araucaria* has also Wealden representatives in the form of wood described under the generic name *Araucarioxylon*, and in leaf-bearing twigs and cones. Some of the coniferous twigs described under the names *Pagiophyllum* and *Sphenolepidium* are, no doubt, fragments of Araucarian trees, and it has been suggested that certain examples of *Sphenolepidium* are near allies of the recent genus *Sequoia* now confined to California. The genus *Brachyphyllum*, another common Wealden conifer, bears a remarkably close resemblance in the formal and stiff habit of its branches with small adpressed scale-leaves to some species of the archaic-looking Tasmanian genus *Athrotaxis*. Specimens referred to *Thuytes* and other genera may be regarded as representatives of the recent Cupressineæ; and the genus *Nageiopsis*, especially characteristic of the American Potomac beds, and represented by one or two fragments from an English locality, may be a near relative of some of the recent species of *Podocarpus*. It must be remembered, however, that the leaves of the Australian *Araucaria Bidwillii* Hook. agree very closely with those of the fossil *Nageiopsis*, and this genus must be left for the present as one of doubtful systematic position.

From Germany and elsewhere, but not so far from any English Wealden locality, leaves have been described which are practically identical with those of *Ginkgo biloba* L., the maiden-hair tree, an aberrant and almost extinct member

¹ Barber. ² Knowlton, p. 46, pls. ii. and iii. ³ Carruthers, Seward (3).

of the Coniferæ, which was formerly widely spread throughout the American and European continents in Mesozoic and Tertiary times.

Numerous other genera which might be compared with more or less probability with existing types, need not be considered; nor can we discuss the various examples of Wealden plants which possess certain features of interest but are of no special importance from our present point of view.

In summarising our knowledge of such Wealden species as may be referred with a considerable degree of certainty to existing families or genera, the following may be mentioned :—

CHAROPHYTA.—*Chara Knowltoni* Sew. *C. Jaccardi* Heer.

BRYOPHYTA.—*Marchantites Zeilleri* Sew.

PTERIDOPHYTA.—

1. EUISETACEÆ.—*Equisetites Burchardti* Dunk. *E. Lyelli* Mant.

2. FILICINÆ.—

(a). Polypodiaceæ. *Onychiopsis Mantelli* (Brongn.). *O. elongata* (Geyl.).

(b). Matonineæ. *Matonidium Göpperti* (Ett.) ? *Microdictyon Dunkeri* (Schenk).

(c). Cyatheaceæ. *Protopteris punctata* Sternb.

(d). Gleicheniaceæ. *Gleichenia* sp.

(e). ? Schizaceæ. *Ruffordia Göpperti* Sew.

CYCADALES.—

1. BENNETTITEÆ. *Bennettites Saxbyanus* (Brown). *B. (Williamsonia) Carruthersi* Sew. Also several American and other species.

2. ? CYCADACEÆ.—*Yatesia Morrisii* Carr. *Bucklandia anomala* (Stokes and Webb). *Fittonia Ruffordi* Sew. *Androstrobus Nathorsti* Sew. *Cycadeoidea gigantea* Sew. (This species of Purbeck age agrees with *Bennettites* in the possession of the characteristic fern-like rammenta on the petiole bases, but there is no evidence of any lateral shoots bearing flowers.)

The numerous species of cycadean fronds cannot be assigned with any certainty to one or other sub-division of the Cycadales.

CONIFERÆ.—*Pinites Ruffordi* Sew. *P. Solmsi* Sew. *P. Dunkeri* Carr., etc. *Araucarites* sp. *Araucarioxylon* sp. *Thuites* sp. *Brachyphyllum obesum* Heer. *B. spinosum* Sew. (cf. *Athrotaxis* sp.).

The most striking fact revealed by a general review of the Wealden flora of England, Germany, and most other regions is the absence of any genera which can reasonably be referred to that great class of flowering plants which constitutes so large a proportion of present-day vegetation. It is true that certain fossil angiospermous leaves have been described by Fontaine¹ and Lester Ward² from the lower members of the Potomac formation which contain plants of a distinct Wealden facies, and by the late Marquis of Saporta³ from rocks on a corresponding horizon in Portugal. The published drawings of some of these so-called archetypal Angiosperms are highly suggestive of fern leaves with reticulate venation; and while admitting the possibility that some few fossil leaves from true Wealden beds may be those of dicotyledonous trees, I believe it would be too rash a statement to make that we have so far discovered undoubted Angiosperms in the Wealden flora.

In connection with this question of the first appearance of angiospermous leaves, I may mention that there are two fairly clearly preserved leaf-impressions, which appear to be those of Dicotyledons, in the British Museum collection of fossil plants from the Stonesfield slate of Oxfordshire. The plants from this lower horizon (Great Oolite) are now being revised with a view to a comparative treatment of the Stonesfield flora.

As regards the ferns of Wealden age, the absence of any species which afford evidence of Marattiaceous affinities is a point of some interest. In the Palæozoic vegetation the small tropical family of the Marattiaceæ appears to have been abundantly represented, but in the Mesozoic epoch, at least in the more recent formations, this division of the Filicineæ occupies a position much more in accordance with that which it holds to-day. A small fragment of a sterile pinna from Portugal has been referred by Saporta to the genus *Marattia*,⁴ but it is difficult to understand on what grounds the name has been adopted. Another fact which is illustrated by the Wealden flora, and in a still more

¹ Fontaine.² Ward (3).³ Saporta.⁴ *Ibid.*, p. 83, pl. xvi., fig. 14.

striking manner by Mesozoic floras of a slightly different age, is the wide distribution and abundance in the European vegetation of species of the *Gleichenia* type. Species of other genera similarly throw light upon the wider distribution and greater abundance of ferns now confined to the tropics.

The chief interest connected with the Wealden cycadean flora is the abundance of the Bennettiteæ, plants which flourished for only a short period during the Jurassic epoch and the beginning of the Cretaceous period. This is not the place to describe in detail the various morphological features of the genus *Bennettites*;¹ it is a type of plant which had no doubt a common ancestry with the true Cycads, and in the structure of its female flowers displayed a nearer approach to angiospermous characters than is the case in the reproductive organs of the Cycads. The Bennettiteæ probably represent a branch of the cycadean phylum, which cannot be traced to any direct offshoot among existing types.

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A. C. SEWARD.

APPENDIX.

NOTICES OF BOOKS.

An Introduction to Human Physiology. By Augustus D. Waller, M.D., F.R.S. Third Edition. 1896.

We congratulate Professor Waller on the appearance of a third edition of this work. An interval of barely five years extends between the first edition and the present. The book has been a pioneer in several ways, amongst others in the limitation of the scope of an English manual on physiology, written avowedly for the use of students of medicine, to physiology alone without the addition of extraneous histological description. As a complete text-book of moderate dimensions on human physiology we regard it as the best in the language. Nor do we know of any so good either in French or German.

Physiological problems are not easy to investigate with success, neither is the science one that lends itself to being "embracéd with an easy span". At least as difficult is it to convey within the compass of 600 octavo pages a comprehensive survey of the physiology of man. The physics and chemistry of animals being a large subject, the medical curriculum with right-minded modesty demands only a part of that whole. That part, however, the part concerning man, is and certainly for long will be the larger fraction. "The proper study of mankind is man," and his physiology includes the brain as an organ of mind. To all this larger fraction—and in reality to much else besides—Professor Waller's book is excellent "guide, philosopher, and friend".

In a text-book of not immoderate dimensions there are obviously certain lines only on which a science can be traced. The historical method is scarcely admissible in a treatise of that size. Approved facts can be judiciously selected from the whole mass available, and these catalogued will give a monument of evidence so far as they go. They are however not the study itself but only the subject matter of the study. Moreover, the inductive is only part of the method pursued by physiology. Leaning on the exacter and more mathematical studies of physics and chemistry it uses laws revealed by them—instance that of the conservation of energy—as instrument for analysis of the corporeal machines which form its subject. Besides facts therefore generalisations, wide and limited, must also be deposited in the text-book. The question is for every volume, how many and how far? In text-books it is too current not only to *catalogue* facts but also to *catalogue* generalisations. The small manual of physiology usually devotes a line to each doctrine, the name of an authority being thereto appended label-like. This is honesty, but it is not responsibility, and from the latter no mind teaching another mind may shrink. What wonder under such

lapse of duty if by the student, even though the label be remembered, that to which it belonged soon fade forgotten.

In the book before us it is pre-eminently the generalisations, the opinions, of his science which the writer sets himself to tell, and in no instance does he fail to unfold each in the manner of a thesis to be defended in the presence of impartial but of critical inquirers. Pursuing that course his facts come forward duly as items of evidence ordered in natural sequence for a logical mind. It is in direct proportion as this can be done with any study that it can claim to possess educational value. It is only when such a method is pursued that the student grasps the proportion of the unknown to the known. It is only in such a teacher that the student acquires that confidence which loyalty of acknowledgment of want of knowledge is one of the most powerful means to inspire. No one can lay down the present book without realising that its subject science, altogether apart from its technical applications to practical medicine, is replete with value as an educational means, and is expounded in such a manner as to fit it for that end. For this Prof. Waller should have the thanks of all, especially of English physiologists, for certainly no single volume of moderate compass in the language had realised this aim. Physiology, although it is a truism to say that it forms the basis of the art of healing, is too often thought of merely as an appanage of medicine. But physiology has a wider aim than that. It will "raise its voice, not only in the hospital and consulting room, but in the school and in the senate". Changes in what we call the body bring about changes in what we call the mind. History, as surely as she prophesies a fuller and more exact knowledge of that molecular dance, which is the material token of nervous action, prophesies to physiology a place of appeal and law-giving in questions not only of the body, but also of the mind. To this no stronger witness can be pointed than all the latter part of Prof. Waller's chapter xv. Though this book is written avowedly for the student of medicine, it is an admirable introduction to human physiology for any student bent upon that knowledge for its own sake only. Indeed the chief evidence that the author has addressed himself in particular to the medical student is the insertion in the volume of a certain amount of elementary physics at the expense of space that might have contained physiology. This is no fault of the author's. The teacher of physiology has early to recognise that he has usually to deal with students who know little or nothing of physics. The regulations of the great diploma-granting Conjoint Board in the metropolis requires no general course of instruction in physics as a part of its course of education for the physician and the surgeon. The teacher before beginning to lecture upon muscle, in treating of which he must perforce employ electricity, is obliged to explain the nature of the voltaic cell, of the induction coil, and of the general facts regarding resistance, and the detection and measurement of currents. Similarly, his microscopical analysis of muscle-structure has to be prefaced with a statement

of facts regarding polarised light and the construction of polarising apparatus. Were a course of instruction in general physics obligatory at the beginning of the education of all medical students, the case would be different. But the only person concerned with his education who does require of the diploma student a certain knowledge of physics is his physiological teacher. Hence a number of Prof. Waller's pages are occupied with statements belonging to elementary physics and with descriptions of ordinary simple physical instruments. These are admirably given; but probably no one realises more vividly and regretfully than their author the sacrifice they necessitate of rightful physiological matter, for it is he who is best acquainted with the keenness of competition of rival matters for every paragraph of a text-book like the one he gives us.

The tone of utterance of Prof. Waller's book is piquantly dogmatic. It is condensed not by abbreviation of construction so much as by epigram. Brevity is the soul of wit. In other words, compression of thought engenders force of thought. In physiological phrase, contrast emphasises words and ideas as it does colour-sensations. For a teacher to be successful dogmatism is unavoidable; he must be dogmatic. There are whole pages in this book that teach almost in epigram. This may be dangerous. Epigram is akin to paradox, and no paradox was ever either the whole truth or nothing but the truth. Nevertheless it is part of the superlative value of this book, for it attracts and interests and incites inquiry, in short captivates for an earnest science a new student lover. The book is indeed that rare thing—a serious scientific manual possessing literary style. Its fertility of expression has begotten phrases that have already become dear to the examinee. The student in *viva voce* replies, making one start, "The appetite for special centres has lost its edge"; or says blandly, "The hypnotised person is one in a state of suggestibility". He has often much of this Wallerian manner in him, and let us hope its matter is with him as abiding knowledge.

The new matter introduced into this edition of the book is chiefly contained in that portion which deals with nerve and retina, and the chapter on Animal Electricity. The results of the author's comparative researches on the influence of vapours, especially of anæsthetic vapours, on nerve, are succinctly related and illustrated by seven new original figures.

The book before us has yet another value for the student of medicine. It is the book latest placed in all his curriculum which possesses first-rate educational as well as technical value. It presents worthily the most philosophical, the least empirical, knowledge that his profession demands of him. The exercises of human anatomy, devoid of scientific quality, involve mnemonic gymnastics of a kind, but as intellectual training they are probably worse than useless. The apology for the time spent on human anatomy is that of the catalogue of minute facts composing it. Some are of importance to the applied

technique of healing. On the contrary, by the volume before us the whole scientific basis of medicine is introduced. How a student giving a fraction of his time for a year can be supposed to conscientiously master its contents is a matter that will strike any practical person who turns over the pages of the book. The ordinary student cannot possibly do so, and a change must be made for him. When it is, nor is it apparently long distant, we hope Prof. Waller's *Human Physiology* will even more generally come into use than at present. The book is largely read already we know by University students, but it is not sufficiently used or examined upon in the hospital medical schools because there diploma students are in the majority. The volume is capitally illustrated. The figures are for the most part original, and even of those borrowed not a few have received signal improvements for a teaching purpose, *e.g.*, Goldscheider's, fig. 262.

No flattery can be more sincere than that which takes the form of imitation. The influence which this text-book, since the appearance of its first edition several years ago, has exercised on English physiological manuals must, to its author, have been amusing if no more. The result has been most beneficial to our students. We congratulate him on having borne his pleasure in silence.

It is difficult to single out passages for special commendation. Did we attempt it, one taken would be the summary of hypnosis, only four pages long, and yet containing everything wanted. Occipito-retinal correspondence, and the masterly paragraph on the place of psychological terms in physiology, would be others. The whole chapter on animal heat is very excellent. Reference might with advantage be made to Dr. Haldane's gravimetric method of estimation of the respiratory exchange. Have the pages been re-indexed for this edition? We would also urge the addition of a centimetre scale to the measures given in the appendix. It is not easy to realise the difficulty which the English student has in visualising the to him still unfamiliar metric units.

We believe that a French translation from the present edition has been undertaken by Professor Herzen of Lausanne and will be published by MM. Masson, Paris.

In conclusion, we repeat that this *Introduction to Human Physiology* is, of all the manuals of moderate size which we know on the subject, the best. It is wholly admirable. We hope it will succeed more and more in displacing other and smaller text-books from the field, for its victory will be a reliable sign of educational progress in this country.

The Calculus for Engineers. By John Perry, M.E., D.Sc., F.R.S.
Edward Arnold. Price, 7s. 6d.

This book cannot fail to prove of great use to a large class of readers. In his introductory remarks the author states that he writes

more particularly for readers who have had very little Mathematical training, but that he also has in view readers who already know a good deal about the Calculus, but are unable to apply their knowledge in practical Engineering problems: "A man learns to use the Calculus as he learns to use the chisel and file on actual concrete bits of work, and it is on this idea that I act in teaching the use of Calculus to Engineers".

The powerful tools of the Calculus are put into the hands of the reader, and he is shown to what use they may be turned in the solution of Engineering problems.

The first chapter of more than 150 pages is devoted to the study of x^n . The use of squared paper is explained, and a set of exercises on graphing are given: also the method of investigating whether any empirical law such as $y = \frac{ax}{bx + c}$, or $yx^n = \text{constant}$, connects two variables x, y , of which sets of simultaneous values have been experimentally determined. By means of numerical illustrations the idea of a limit is clearly shown, and the absolute accuracy of a differential coefficient as a rate-measurer brought home thoroughly to the mind of the reader. The arithmetical method of finding approximately the acceleration at different points of a moving body whose positions at close intervals have been observed, is illustrated in a table.

The integration of $\int x^m dx$ is immediately defined as the inverse of differentiation, and here perhaps it might have been well to have shown at length that $\int_a^b F'(x) dx = F(b) - F(a)$

Partial differentiation, maxima and minima, and tangents and normals to a curve are briefly explained, and then areas of closed curves, lengths of curves, volumes of surfaces and moments of inertia are dealt with, and a large number of illustrative problems on the bending of beams, the flow of liquids, magnetism, and thermodynamics, are discussed, nearly all of which require no further knowledge of the Calculus than the differentiation and integration of x^m .

Chapter ii. deals with the compound interest law and harmonic functions, that is with problems involving the functions e^x and $\sin ax$, a quantity whose rate of increase is proportional to itself being said to follow the compound interest law. Here again numerical illustrations of the value of the differential coefficients are recommended to the consideration of the reader, and many electrical and mechanical problems are discussed. Fourier's theorem is stated, and its practical application is explained. On the subject of vibrations a very instructive numerical example of a linear differential equation with constant coefficients is worked out at length.

Chapter iii. is called "Academic Exercises," and contains in brief some of the usual elementary portions of the ordinary treatises on Differential and Integral Calculus and Differential Equations. Elliptic

Integrals are just touched upon, also spherical harmonics, Bessel's function, hyperbolic functions and gamma functions. The chapter closes with a problem on the conduction of heat, and a tabulated list of integrals.

The book is printed in clear, good-sized type, on good paper, answers are in nearly all cases given to the exercises, and an index is provided.

To the teacher of the Calculus, no less than to the Engineer, this book will prove of great value as a rich storehouse of practical illustrative examples, by means of which he may impart to his students a real useful working knowledge of the subject.

Vorlesungen ueber Bakterien. Von Dr. Alfred Fischer, a.o. Professor der Botanik in Leipzig. Mit 29 Abbildungen. Jena: Verlag von Gustav Fischer, 1897.

All who have followed Prof. Fischer's work on Bacteria will open his new book on these organisms with anticipatory pleasure. Nor will they be disappointed. Since the publication of Dr. Bary's fascinating lectures, nothing has been written concerning Bacteria, which gives so clear and interesting a sketch of the present state of our knowledge in this department of Botany as does Dr. Fischer's new book. With the advance of knowledge, much of what we used to accept as fact has been found to require revision, and not only have many of the old puzzles been gradually solved, but many new paths of inquiry have been opened up. Winogradsky's magnificent researches are ably summarised, and the results, showing that some bacteria possess the power of assimilating the carbon dioxide from the atmosphere, others again that of obtaining their energy by oxidising sulphuretted hydrogen, first to water and sulphur and finally to sulphur dioxide, instead of relying on the oxidation of carbohydrates, are presented in a clear and readable form.

The bacteria concerned in the fixation of nitrogen and the relation of the process to the carbohydrates obtained from the leguminous plants, in which they engender the formation of tubercles, receives a clear though somewhat brief exposition.

Some of the other instances of symbiosis however are not so happily chosen. In a book of this kind, a discussion on the symbiotic relations of fungus and alga in lichens seems to be somewhat out of place, and the beautiful case of the gingerbeer plant, so thoroughly investigated by Marshall Ward, might with advantage have been substituted. As it is, this, perhaps the most striking example of symbiosis of its kind, finds no mention at the hands of our author. The later chapters, dealing with the relation of bacteria to disease are perhaps less interesting than many of the preceding ones, but within the compass of a short course of lectures, such as that of which the volume before us is made up, possibly this is not easily avoidable.

Dr. Fischer adds another to the steadily growing lists of proposed classifications, and he attaches considerable importance in delimiting his genera to the absence or the presence of cilia, and to the mode of arrangement of the latter when they exist.

As an important contribution to the literature of Bacteria, the book will form a necessary addition to the library of all who are interested in these organisms, while even the general reader will find a great deal which cannot but be of great interest to him.

Das Kleine Botanische Practicum für Anfänger. Dr. Ed. Strasburger, o. ö. Professor der Botanik an der Universität Bonn. Dritter umgearbeitete Auflage. Mit 121 Holzschnitten. Jena: Verlag von Gustav Fischer, 1897.

The smaller edition of Prof. Strasburger's *Practicum*, like the larger work which is so widely known, has passed through another edition, and we notice several improvements in the volume in its present form. The number of types is reduced, and all will probably agree with the author in recognising that less done thoroughly is better than more done superficially. As would have been expected, the new edition is thoroughly—especially as regards the latest work on nuclei—up to date, and is admirably arranged, not only to suit the learner but also to help the teacher.

Bau und leben unserer Waldbäume. Von Dr. M. Büsgen, Professor an der grossherzoglich sächs. Forstlehranstalt in Eisenach. Jena: Verlag von Gustav Fischer, 1897.

The object of the volume before us is to give a semi-popular account of the structure, physiology and general mode of life of our forest trees. Such a book is not a very easy one to write, and we can hardly say that Dr. Büsgen has quite succeeded in his task as a whole. Indeed, we must confess to finding his book a trifle dull. It contains, however, some useful summaries of views which have been advanced as explanations of certain problematical phenomena; thus the mode of the formation of annual rings is briefly but fairly discussed in five pages, and a tolerably good account is given of the contributions made by Dixon and Soly and by Askenasy towards the solution of the old-standing riddle as to the ascent of sap in tall trees.

Applied Mechanics, a treatise for the use of students who have time to work experimental, numerical, and graphical exercises illustrating the subject. By John Perry, M.E., D.Sc., F.R.S., Professor of Mechanics and Mathematics at the Royal College of Science, South Kensington. With 371 illustrations. Pp. viii., 678. Cassell & Co., Ltd. 1897.

In this book Prof. Perry gives the substance of the lectures on Applied Mechanics he has for many years delivered at the Finsbury

Technical College, the portions of the subject which are suitable for an elementary course to first year students being printed in larger type than the more advanced portions.

In addition large collections of numerical and graphical exercises (with answers appended), are given at various stages in the development of the subject. These will prove very useful both to teachers and students.

One of the most interesting features of the book is the description and illustration of several pieces of laboratory apparatus, which have been designed by Prof. Perry for students' use in performing quantitative experiments in Applied Mechanics.

In fact the professor takes up strongly the position of a reformer of the "academic" methods of instruction in Applied Mechanics which he conceives to be in vogue in too many places, and insists on the absolute necessity for students making quantitative experiments in a Mechanical Laboratory as an essential part of any proper course of instruction in Mechanics. He refers with just pride to more than twenty complete sets of his apparatus which have already been made for various institutions, and no doubt the publication of this book will induce many other colleges and technical classes to follow more or less closely his system of instruction.

Prof. Perry acknowledges the pioneer work of Prof. Ball at the Royal College of Science, Dublin, in initiating quantitative experimental work in illustration of the principles of Mechanics. But instead of building up each piece of apparatus out of a small number of elements, which served for many different experiments, Prof. Perry's plan has been to use a distinct piece of apparatus for each experiment (with the obvious advantage of having the apparatus always ready for use), and instead of the lecturer performing the experiment the "student measures things for himself; illustrates mechanical principles; finds the limits to which the notions of the books as to friction and properties of materials are correct; learns the use of squared paper, and the accuracy of graphical methods of calculation; and, above all, really learns to think for himself".

Another excellent feature of the work before us is the freshness of the information and the originality of method obvious on nearly every page. Whilst many teachers will consider the style too polemical for a text-book, and will look on some of the attacks on what are here called "academic" methods, and "academic" persons as uncalled for and quite unnecessary in a "treatise for the use of students," no one interested in the training of mechanical engineers can afford to neglect the results of Prof. Perry's experience as embodied in this treatise.

In regard to the "get up" of the book the illustrations with some few exceptions are very satisfactory, but the paper is too thin, or rather is not sufficiently opaque, and this makes the small type very trying to the eyes; the printing also of the mathematical portions leaves much to be desired.

The importance of the book is such as to justify the hope that the above defects may soon be remedied in a new edition.

Theoretical Mechanics, an introductory treatise on the Principles of Dynamics, with applications and numerous examples. By A. E. H. Love, M.A., F.R.S., Fellow and Lecturer of St. John's College, Cambridge. Pp. xvi., 379. Cambridge University Press, 1897.

The author of this book furnishes mathematical students with an introduction to dynamics which will probably have some considerable influence on the progress of science. For instead of slavishly adopting the principles of Mechanics as laid down by Newton, Mr. Love has not shrunk from independent treatment, and has taken great pains to expound the subject in strict accordance with the best modern ideas.

The language throughout is very carefully chosen. As an example of the way in which precision of statement is attained, we may mention the explicit recognition of three classes of vectors marked by different degrees of localisation: unlocalised vectors, vectors localised in a line, and vectors localised at a point; where in the first class the line representing the vector may be drawn from any point, in the second class from any point in a particular line, whilst in the third class the line representing the vector must be drawn from the specified point.

Instead of the artificial "effective force" which has been so long employed in connection with D'Alembert's principle, the term "kinetic reaction" is adopted. At first glance "kinetic reaction" strikes us as a happy abbreviation for Newton's "reaction against acceleration," but as used in this book it designates the vector localised in the same line and having the opposite sense.

Throughout the work the physical aspects of the subject are kept prominently in view, and the order of difficulty of the physical notions involved determines the arrangement of the several parts of the subject matter. The first part of the book accustoms the student to the idea of acceleration. In the second part the notion of mass is the central idea, and here the general principles of dynamics are expounded and the equations of motion formulated; the theory of work and energy is dealt with in the last chapter of this part. The third part gives the methods of applying the general theory to different classes of problems. First come two chapters on dynamics of a particle, one on free motions, the other on motion under constraints and resistances. Next follows a chapter on motion of a rigid body in two dimensions. This is succeeded by an important and very suggestive chapter on impulses, initial motions, small oscillations, stability of steady motions, the motion of chains, and some further applications of the principles of energy and momentum to systems of rigid bodies.

The concluding chapter contains a critical discussion of points which involve special difficulties when treated from the standpoint of the modern doctrine of the relativity of motion.

An important feature of this book is the series of illustrative problems which are worked out fully in the text.

Large collections of examples are appended to several of the chapters. These examples are carefully grouped, and afford considerable opportunities of choice to teachers and students making this their text-book.

Untersuchungen ueber Bau Kerntheilung und Bewegung der Diatomeen,
von Robert Lauterborn. Mit 1 Figur im Text und 10 Tafeln.
Leipzig: Verlag von Wilhelm Engelmann, 1896.

This splendidly got up work reflects credit no less on the author than the publisher. The Diatoms have been too long the almost exclusive property of the amateur, but the important results which have been obtained by Dr. Lauterborn can hardly fail to direct new attention to this group of organisms.

After a brief historical account of the work of previous investigators, followed by a general description of the methods which he employed, the author passes on to the results of his researches on various members of the Diatomaceæ, and these results are as surprising as they are novel. Dr. Lauterborn has wisely checked his investigations on the killed and fixed plants, by observations made on living material, and he has also applied the microtome methods with great success in his endeavours to elucidate the anatomical structure, often very complicated, of the Diatom cell. Thus in *Pinnularia*, the "raphe" is shown to represent an open slit which places the protoplasm in communication with the external world, and in *Surirella calcarata* the same end is attained by means of slits which occur at the edges of the four wing-like outgrowths so characteristic of this species.

As regards the structural characters exhibited by the living contents of the cell, Dr. Lauterborn has arrived at several remarkable conclusions and his results will, if confirmed, prove of great importance. For in these organisms, representing a somewhat isolated phylum, only joining the main line somewhere amongst the lower algæ, we meet with a protoplasmic complexity for which we seek in vain amongst the higher animals and plants, finding at most a faint degree of resemblance amongst the protozoa. And, indeed, it is a remarkable fact, if we take a general survey of organic life, that we find the mechanism of cell-division so extremely complex amongst these lower forms, often far more so than is the case with plants or animals far higher up in the evolutionary scale.

Amongst the observations recorded on the mode of nuclear division, Lauterborn mentions that he was able to recognise cross fibrils of protoplasm connecting the aster rays, and he adduces a considerable body of evidence in favour of Bütschli's theory as to the foam-structure of protoplasm. Even in the nucleus, in some instances, he asserts that a foam-structure can be distinguished, the chromatin particles occurring in the angles where the foam-walls meet. The centrosomes are stated

to be readily discerned and often to be clearly recognisable during life, but it must be stated that, judging from the figures, they do not very closely resemble the centrosomes of such higher plants and animals in which these bodies have been clearly identified.

The most remarkable statements in the book are those which refer to the origin and structure of the spindle. This body is said to arise from the centrosome as a rounded structure, which eventually assumes a cylindrical form, and then penetrates the nucleus, travelling towards it as a perfectly definite entity. When it reaches the interior of the nucleus it increases in size and the chromosomes ultimately become arrayed around it, in a manner similar to that which has been described for some protozoa. The original centrosome, after having budded off the spindle, finally degenerates and new ones are formed from the ends of the spindle towards the close of mitosis. It is suggested that the daughter-chromosomes are attracted chemotactically towards the poles and are not pulled thither by the contraction of a peripheral mantle of spindle fibres. After the separation of the constituents of the two daughter-nuclei, a cell-wall gradually advances from the periphery, cutting the entire cell into two parts in essentially the same fashion as has been described by Strasburger and others for, *e.g.*, *Spirogyra*.

After the account of the process of nuclear division, which is illustrated by many beautiful figures, the author proceeds to discuss the mode of movement prevalent amongst Diatoms and concludes that locomotion is effected by means of the emission of a mucilaginous substance through the slits in the shell.

Enough has been said to show that the book teems with new observations and it is to be hoped that other investigators may also be attracted to so promising a field of research as the Diatoms clearly afford, and such is also the desire expressed by the author himself, who thereby proves himself to be superior to that commercial instinct, occasionally to be met with even amongst scientific men, which prompts them to keep to themselves any promising lines of investigation on which they may have lighted lest some one else should happen to infringe on their paltry claims to priority.

APPENDIX.

NOTICES OF BOOKS.

System der Bakterien. Handbuch der Morphologie Entwicklungsgeschichte und Systematik der Bakterien. Von Dr. W. Migula, a.o. Professor an der technischen Hochschule zu Karlsruhe. Erster Band, mit 6 Tafeln. Jena: Verlag von Gustav Fischer, 1897.

Dr. Migula has earned the gratitude of those who, unable by reason of the pressing claims of other work to cope with the hordes of original contributions so rapidly following on each other's heels, yet desire to be placed *au courant* with the main lines of advance in bacteriology. He has not attempted an easy task. The mere working through the steadily increasing literature must have required an immense amount of perseverance, but he has succeeded in giving his readers a fair account of a many-sided subject. Opening with an historical introduction, occupying the first fifty pages or so, he proceeds to give a fairly detailed discussion on points connected with the morphology and development of the organisms themselves. The methods of growing the plants, and the varied conditions of their life, including their influence on the substratum, occupies the concluding chapters of a volume so full of information as to cause one to express the hope that its promised successor will appear as soon as may be. Dr. Migula is decidedly to be congratulated on this first instalment of an important work.

Views on Some of the Phenomena of Nature. By James Walker. London: Swan Sonnenschein & Co., 1898.

The general character of this book is sufficiently set forth in the following extract, taken from p. 187:—

“Light is the combined PLASMA of all the several substances which are in the sun's pyrosphere in an incandescent state, and carried off by a flood of force generated in, and ejected from the sun, and which combination constitutes ELECTROGENE”.

A Text-book of Zoology. By T. Jeffrey Parker, D.Sc., F.R.S., and William A. Haswell, M.A., D.Sc., F.R.S. In two volumes. Pp. lvii., 1462. With 1173 illustrations. Macmillan & Co. Price 36s. net.

The appearance of this important work, for which many of us have been eagerly waiting, will be hailed with pleasure, but not, alas, unmixed with sorrow. For almost simultaneous with its publication we have to record the premature death of one of its authors. The late Prof. Parker's earlier works are well known and highly appreciated by all students and teachers, and we believe that the present book, representing as it does the finishing and crowning work of a life devoted to the furthering of biological science, will attain to an equal if not to a greater degree of popularity than that enjoyed by his previous works.

While we cannot avoid attaching this melancholy interest to the book before us, in connection with one of its authors, we must, on the other hand, congratulate Prof. Haswell on the completion of his great task.

Although the scientific papers of this author have long been known to the specialist and advanced student, till now Prof. Haswell has been comparatively unknown to the elementary student in this country.

When we look at the latest editions of our scientific text-books we see all of them steadily increasing in size; this must be inevitably the case with all branches of science that are actively growing. Comforting as these signs of activity may be to the advanced student, the beginner is likely to be terrified when he sees the number of pages which he has to master even to make himself familiar with the rudiments of a science. The present work, we are afraid, will not reassure him in this respect, for it consists of no less than 1400 pages, and yet, as the authors state, is strictly adapted to the needs of a beginner. One must, however, remember that the 1200 illustrations considerably reduce the bulk of the text and renders the same clear and easy of comprehension. Still we cannot help thinking with regret that the large size of this work, and consequent large price, will keep it out of the hands of many of our elementary students who stand in great need of a good text-book of zoology.

For a text-book of zoology our authors pursue a novel course and one that will undoubtedly commend itself to the beginner with no previous knowledge of the subject. An example of each important class is carefully considered in detail before proceeding to formulate the distinctive characters of the group, then follows the classification and a consideration of the chief variations in structure and development met with in the class, and, finally, a paragraph dealing with the inter-relations of the different orders, this being in all cases terminated with our old and much-abused friend the phylogenetic tree. Many will no doubt object to these "diagrams illustrating the mutual relationships of the class," but, after all, they are almost necessary for the beginner and serve a useful purpose if the latter will only study the conclusions upon which they are based, and remember that they are only attempts to interpret the known facts of morphology, embryology and palæontology.

For the benefit of the beginner the subject is introduced by a chapter on structure and physiology, and here we must put forward a strong protest against the unfair treatment which the spermatozoon meets with, thus we find it stated that the Metazoa originate from a single cell, the ovum, thus ignoring the cell-value of the spermatozoon and regarding the latter merely as the impulse to development. Surely sufficient is now known of the development of the two sexual cells to prove that the ovum and spermatozoon are morphological equivalents and that the ovum, save in the still little-understood parthenogenesis, can no more give rise to an embryo by itself than can the spermatozoon. The equal value of these two elements might be demonstrated even to the elementary student if the last two divisions of the sperm-cell had been given, in addition to the maturation of the ovum, which, taken alone, is almost inexplicable to the beginner. In this same chapter we find Fol's "Quadrille of the Centrosomes" cropping up again, notwithstanding the fact that Boveri and others have long shown that theory to be erroneous. We sincerely hope this will be removed from the next edition and a less prominent rôle assigned to the centrosome, which is

at last deservedly on the decline, many even of its continental advocates, now show signs of want of faith in its omnipotence.

Botanists, we are afraid, will take exception to the excellent chapter on the Protozoa, where we find such forms as *Hæmatococcus*, *Pandorina* and *Volvox* included as animals. The Mycetozoa are very aptly placed with *Protomyxa*, with which genus they show great similarity in their life cycle. We presume that in the arrangement of the sections the authors pursued some definite plan, but, if so, we utterly fail to fathom the reason for the intercalation of the Echinodermata between the Molluscoida and the Annulata, by this means the Trochelminthes and Molluscoida become completely separated from the Annulata, with which group even our authors consider the Trochelminthes, at least, to be related, as may be seen from the diagram on p. 483. The inclusion of *Phoronis* amongst the Molluscoida is open to grave doubt; and we fail to see why doubt should be thrown on the account of the development of the Endoproctous Polyzoa so ably given by Harmer, and the Ectoprocta, whose developmental history is fairly well known, interpreted as if they developed like *Phoronis*. Of course these forms are particularly difficult to classify in a natural manner, but we do not think that the plan adopted here will commend itself to any who have worked at these groups. The retention of the *Echinridæ* as an order of the Gephyrea is very antiquated and unnatural, for neither in their development nor in their adult structure do they show much resemblance to *Sipunculus*, and they find a much more natural position with the Chætopoda.

It is quite delightful to see such a large number of new illustrations, and as, in many cases, there is a complete description of the anatomy of some hitherto little-known form, now copiously illustrated, these will be of the greatest value even to the most advanced student. Amongst these we would especially draw attention to the description and illustrations of the anatomy of the Starfish (*Anthenea acuta*), of *Nereis*, of *Triton nodiferus*, and of *Nautilus* amongst the invertebrata, while among the vertebrata we find excellent accounts of fish which to us are rarities, viz., *Callorhynchus* and *Chiloscyllium*.

Judging from the statements in the preface (p. viii.) we should not have expected our authors to adopt any views which had not stood the test of time, but in dealing with the mammalian dentition we find them adopting the unsupported views of Wilson and Hill, which are at variance with all those of the numerous European workers, many of whom have spent years in the study.

The wealth of illustrations is wonderful, and these have for the most part been judiciously chosen, but a few are not quite so happy. Fig. 453, for instance, is a shocking misrepresentation of a cockroach, and would certainly give the beginner the idea that the wing cases were attached to the pro- instead of to the meso-thorax; fig. 510 also is misleading, and the elementary student who had never seen a *Limulus* would certainly receive the impression that this animal had a pair of large compound eyes situated close to the middle line. Surely, too, our authors have made a slip of the pen in labelling the figure of a mosquito as *Pulex*, or has some industrious systematist been digging in the dust-

bins of the past and found a fresh name for the gnat. Systematic terminology is always a trial to the general zoologist, but whatever terminology is used it should be uniform, and it is a pity to find the same animal posing under two distinct names, as we find the common English earthworm as *Lumbricus herculæus* and *L. agricola*, the latter, according to Benham, being a synonym for the former.

In conclusion, we should like to draw especial attention to the chapters on Distribution, the Philosophy and the History of Zoology, a novel and extremely useful addition to a work on zoology. In the comparison of the animals of Britain and New Zealand it is stated that land Planarians are unknown in the former. Although this is generally thought to be the case it is not really so, a land Planarian having been long ago described from England, and more recently from Ireland. It is, however, not to be wondered at that some small errors should creep into a work of this size; and when we consider the difficulties under which these authors laboured, one in New Zealand the other in Sydney, the printers, publishers and artists in England, one can only marvel at the splendid work they have produced, one that will be useful both to the elementary and advanced student alike as well as to the teacher.

The Outlines of Physics, an Elementary Text-book. By Edward L. Nichols. Pp. 452. New York: The Macmillan Co., 1897.

In his preface the author says that this book is intended as a short course in physics which should be a fair equivalent for the year of advanced mathematics now required for entrance to many colleges. If this is so, then the "advanced mathematics" must be of a decidedly elementary character, for this book is quite elementary.

The work belongs to that class which combines the functions of a text-book with those of a laboratory-manual. The chief objection to the book as a text-book is that on account of want of space the author has hardly been able to treat the subject sufficiently fully for even an elementary class.

The experiments described are in general of such a character that they can be performed without the use of expensive apparatus, and are chosen so that a quantitative result of some kind is to be obtained. In this connection our experience leads us to think that it would have been of much use if the author had devoted a section to the consideration of the method of roughly determining, from the accuracy with which each of the quantities which are actually measured can be determined, the accuracy that can reasonably be expected in the result. The increased importance of the errors made in the measurement of a given quantity if it is involved in the final expression to the second or higher power ought also to be insisted upon, even at the first.

The author being a fellow-countryman of Rowland it seems strange that the value 772 for the mechanical equivalent of heat is given, although it is now known that even Joule's results give, when corrected for errors in his thermometry, a much higher value.

The figures are on the whole excellent, for they are clear and show

how the apparatus is to be put together. There are, however, two conspicuous exceptions in figs. 137 and 147, in which the bends in the glass tubes will certainly incite the beginner to make even a worse pucker than usual. A full-page picture of the tower of Pisa is also hardly in keeping with the other figures.

On the whole the book will be found very suitable for those who require a book which combines the characteristics of a decidedly elementary text-book with those of a good laboratory-manual.

Theory of Electricity and Magnetism. By Charles Emerson Curry, Ph.D. With a preface by Prof. Boltzmann. Pp. 442. London: Macmillan & Co., 1897.

This work embodies the substance of the course of lectures on Electricity and Magnetism given by Prof. Boltzmann at the University of Munich, Dr. Curry having, however, considerably amplified the lectures and added examples and deductions for illustrating the subject matter.

The characteristic of the author's method of treating the subject lies in that starting with the differential equations of the electromagnetic field he then proceeds to deduce from them the explanation of all electric and magnetic phenomena, by means of mechanical analogies, and in this he follows a course quite different from that adopted in the class of text-book in favour in this country. This method of procedure of course necessitates, on the part of the student, a considerable acquaintance not only with the experimental part of the subject but also with the theories of the subject. The work forms in fact an illustration of the standpoint from which the theory of electricity and magnetism is studied on the continent, and will be found of much use to those whose physical and mathematical equipment is sufficiently good.

Starting with the assumption that in every volume-element of the ether a motion exists, and that it is to this motion that electric phenomena are due, and that further the displacement produced by this motion can be represented by a vector which he calls the "tonic vector," the author by making certain assumptions as to the connection between the kinetic and potential energy, etc., and this vector deduces by means of Hamilton's principle the equations of the motion of the ether. By a slight modification these equations are then reduced to Maxwell's form.

In the interpretation of the equations the conception of two incompressible fluids, the positive and negative real electricities, is introduced, together with the idea of free electricity and of electrical polarisation. As an analogy simply meant to illustrate the subject this may be all right, but as a physical theory it is of little use.

After considering Maxwell's form of the equations the author goes on to show by what assumptions and in what manner they may be converted into Helmholtz's form. He also shows how Helmholtz's equations may be deduced from the empirical laws. The subject of Hertzian waves, of the propagation of longitudinal ether waves and of electric and magnetic striction are also dealt with.

The reasoning is on the whole clear and explicit, and the book is remarkably free from errors.

Catalogue of the Fossil Cephalopoda in the British Museum (Natural History). By Arthur H. Foord and George Charles Crick. Part iii. London: Printed by order of the Trustees, 1897.

Catalogue of the Tertiary Mollusca in the Department of Geology, British Museum (Natural History). Part i., *The Australasian Tertiary Mollusca.* By George F. Harris. London: Printed by order of the Trustees, 1897.

Specialists will welcome the appearance of two more volumes dealing with the extensive collections of Fossil Mollusca in the British Museum. The third part of the catalogue of fossil Tetrabranchiate Cephalopoda treats of a family of the Nautiloidea which had not found a place in the first two parts, as well as of the first instalment of the Ammonoidea. The authors agree with Haug in abandoning Fischer's subdivision of the Ammonoidea into Retrosiphonata and Prosiphonata. The classification of the Goniatites adopted is based on that proposed by Prof. Alpheus Hyatt, and the suture lines characteristic of the individual species are illustrated by cuts in the text which will prove invaluable for the identification of specimens by those who are unable to obtain access to the types. It is a matter for congratulation that Dr. Foord has found such an able *collaborateur* and successor in the person of Mr. G. C. Crick.

Mr. G. F. Harris's catalogue is to be the first of a series devoted to the fossil faunas of special geographical regions; dealing as it does with the tertiary mollusca of Australia and New Zealand, with Tasmania and the Chatham Islands, it will supply the student of distribution with a store of facts which will be of the greatest service to him in solving the problem of the past history of the present molluscan fauna. A prominent feature of the work is that the author has devoted especial attention to the growth of shells. The very remarkably perfect state of preservation of the gasterpoda of Victoria and elsewhere has enabled him to show that in many cases distinct modifications of structural detail are mere peculiarities of stages of growth, and are not necessarily of classificatory value. In the introduction the reader's attention is drawn to a few of the more interesting points; but it is a pity that no indication is given to the new names and new species which appear in the work; an asterisk affixed to the names in the index or table of contents would have been of great assistance to the reader or recorder who is desirous of ascertaining the exact contributions which the author considers that he has made to scientific nomenclature. Of great value are the eight excellent plates of illustrations drawn by Miss G. M. Woodward.

Lehrbuch der vergleichenden Mikroskopischen Anatomie der Wirbelthiere. Zweite Teil. Schlund und Darm. Von Dr. A. Oppel, a.o. Professor a.d. Universität Feiburg i. B. Jena: Gustav Fischer, 1897.

During the last two decades discovery concerning the anatomy, histology and functions of the vertebrate alimentary system has

advanced with great rapidity. The work of Edinger and others on the intestinal epithelium of fishes, of Bizzozero and his followers on that of the tubular intestinal glands, the discovery of a true pancreas in the bony-fishes, of pancreatic and splenic tissues in the dipnoi lying wholly within the wall of the gut, and of the compound origin of the pancreas in representative species of the several great groups of vertebrates, together with observations on the functional side no less welcome and important, have brought harmony into confusion and simplified our conceptions not a little, necessitating a stocktaking of the sum of our knowledge in this generally interesting department. To Dr. Oppel has fallen the task of meeting the latter demand, and the present volume is the second of two which he has devoted to the purpose, the first (on "The Stomach") having preceded it by but a year. Contrary to what might be expected, the work thus rapidly issued, is exhaustive, most carefully executed, and thoroughly reliable, and there is evidence of its having occupied its author's attention for several years. Its 664 pages are amply illustrated by 343 well-chosen text-figures and four plates of great excellence. A full bibliography and analytical indexes are given, and the pages of the work are thoroughly salted with references. The whole arrangement is a well-conceived one, whereby the reader is without difficulty enabled to ascertain precisely what has been observed concerning a given species, and where to turn for fuller information upon details. The book is in fact the embodiment of our ideal of the work of reference which we believe the advanced text-book in Science of the future must become. The author has solved the problem of compressing within reasonable limits and presenting in a thoroughly workable form the sum of our knowledge of a vast subject, and that more successfully than most with whose books we are familiar. We note the omission of some few references and of topics which might well have been considered, but all that is leading and important is there. The compilation of works of this order has now become a necessity if energy in the future is to be economised and directed into the channel best conducive to advancement, and to those disposed to enter upon the task of ensuring this, Dr. Oppel's book may well be recommended as a pattern.

The Tailless Batrachians of Europe. Part i. By E. A. Boulenger, F.R.S. London: The Ray Society, 1897.

This book, primarily intended for the English scientific public, deals with a group of animals ever popular and conspicuously associated with the work of English-speaking zoologists. Its 204 pages are illustrated by seventy-seven processed drawings, and nine plates, of which six are coloured, and by two maps; and we have no hesitation in stating that it is one of the most important additions to zoological literature during recent years. It deals comprehensively in its "Introduction" with the Amorous Batrachians, classified according to Cope's system, which all recent investigation has gone to support, and categorically in the eighty-eight pages which follow with the *Discoglossidæ* and *Pelobatidæ*, the latter mode of treatment of the

remaining families being reserved for the second part, now we believe in an advanced state. Habits, structure, reproduction and metamorphosis, and geographical distribution, all receive adequate attention, and are described in popular terms in a manner permitting of ready service in either the field or study; and an enthusiastic keynote struck in the preface pervades the book from cover to cover, author, artists, and all concerned having combined to produce a work which, while marking a new departure of the Ray Society, will perhaps do as much towards encouraging a love of field natural history as any work of its time. The author is notorious for his painstaking enthusiasm as a zoological investigator, even a holiday being to him of no avail unless he have access to the haunts of his favourite animals, and in the pages of this book we find combined with much that is technical and which, as published elsewhere, has helped to render him famous, a vast accumulation of observations made during holiday-time which, while materially extending our knowledge of local distribution and habit, by its very nature appeals the more directly to the outdoor naturalist and passer-by. The weakest portion of the book is the anatomical. The author professedly ignores the anatomy of the nervous, higher sensory, and circulatory systems, and of the soft parts other than the lungs and urinogenital organs, for reasons duly given; but as concerning the latter, confusion for which the author is little responsible appears to have arisen in the description of the "*vesicula seminalis*" *vas efferens* and their alleged relationships to the genital ducts and omission in that of the vestiges of the latter in the male Rana, while in the general anatomical terminology considerable revision might be highly advantageous. When critical, the author brings to bear the full force of his lengthened experience with marked effect, as, for example, in his denunciation of the popular belief that the tadpole metamorphosis is recapitulatory of phylogeny, which we most heartily support. The book is what it professes to be, and by its aid the wayside naturalist should find no difficulty in determining the species and all that is important for taxonomic study of our European forms. To the field naturalist it will be indispensable, and without it no zoological library can be complete. Of all branches of science the zoologists is the most humanising, and it is by works such as that now under review that the mind may be most readily awakened through the beauties to the more philosophic aspects of vital phenomena, the fuller study of which affords a mental discipline second to none other and a means of happiness as inexhaustible as it is beneficial. We eagerly await the appearance of the second part, and regard it a national duty of the Ray Society to provide, if possible, for a third or fourth part, which shall be devoted to the tailed Batrachians, especially since recent study of them has revealed structural and functional conditions unique and unsuspected, such as are pre-eminently calculated to arouse the real appreciation of the instructive in Nature through the gratification of the desire for the sensational and curious, always present in the public mind.

APPENDIX.

NOTICES OF BOOKS.

Practical Acoustics. By C. L. Barnes. London: Macmillan & Co. 1897.

This volume forms the first instalment of the long-promised third volume of Stewart and Gee's *Lessons in Elementary Practical Physics*. To those who have been in the habit of using the previous volumes the present addition will probably prove very disappointing, for the character of the book is entirely different. This is no doubt partly due, as the author explains in his preface, to the difficulty in the subject of acoustics of devising quantitative experiments which can be carried out in a laboratory having the ordinary equipment. One of the chief defects of the book is that it is swamped with the description of experiments which, although they are in many cases curious and interesting, are perfectly unsuited for an educational work. In some cases experiments are described which have not been explained, and which do not illustrate any special portion of the subject. These experiments might have been collected together in an appendix without much harm, but scattered as they are throughout the book, they seem to give the idea that the performance of pretty experiments is the aim and object of the work.

In many places the working or definitions are very obscure. Thus to define the amplitude of a vibratory motion as "The displacement of an antinode from its initial position" seems to restrict the definition to a certain portion of a stationary wave, while the next sentence says, "the mean velocity of each particle is proportional to its own amplitude". The statement on page 27 that the frequency of the transverse vibrations of a string varies directly as the square root of the tension, is followed by the following paragraph: "It also varies directly as the square root of the force of gravity, but this is commonly left out of account". These two statements are perhaps as thoroughly vicious and calculated to confuse the student as any that could be compiled. Again, Young's modulus is defined as the ratio of a small increment of pressure to the resultant diminution of length. In using the word tension when he means stretching force, the author follows a somewhat general practice but one none the less to be regretted.

The book as a whole suffers from undue expansion on the one hand and from looseness of expression on the other, so that although it will probably be found useful for teachers and others in quest of lecture experiments, it is hardly a safe book for the use of the, at any rate the elementary, student of physics.

Dictionnaire de Physiologie. Par Charles Richet, avec la collaboration de MM. P. Langlois et L. Lapique, etc. Tome II., fascicules 2 et 3. Tome III., fascicule 1. Paris: Ancienne Librairie Germer Baillière et C^{ie}. Félix Alcan, Editeur. 1897-98.

One of the most salient features of the above portion of this work is the unequal nature of the articles. It may be that, perhaps, this is due in great measure to the method of production, *viz.*, the entrusting of special sections to special authors. And yet it is a method which, properly carried out, seems capable of giving the very best results, and in text-books and important works on science at the present time the method is being adopted more and more frequently, and often with great success.

Nevertheless it is unavoidable that the method should sometimes fail, and this more particularly if the very fullest instructions have not been provided for the separate authors as to the scope and length of their respective sections, and in the earlier portions of a work like the present one might expect to find a certain disproportion in the articles, but in later parts—and the work has now reached the eighth fasciculus—one is justified in expecting to find more uniformity.

Such however is not the case, and we can only regret the circumstance.

In the article headed "Calcium," occupying seventeen pages, there appears a good discussion of this metal, its occurrence in the organism, its relations to the skeleton in various animals, vertebrate and invertebrate, and its absorption and excretion.

No discussion of its *rôle* in coagulation is given, this being deferred to the articles on Milk, Coagulation, and Blood.

The article on the Cell is particularly disappointing, for we looked forward to finding a thoroughly good discussion of this most interesting subject.

The article occupies only forty pages, and compares very unfavourably with the two very lengthy dissertations on the Brain, which covers 486 pages, or with the Heat, which occupies 190 pages. Moreover, we do not consider that the best use has been made even of this limited space. Section 1 of the article bears the title "Morphology," and occupies three and a half pages. The nucleolus is described as usually occupying an excentric position in the nucleus, as being rounded or oval, and more highly refractive than the latter; as staining more intensely than the nucleus, as comporting itself differently to certain reagents, as having an unknown significance, as disappearing during cellular division to reappear in the young cells, and—that is all!

The centrosome is treated with scarcely more ceremony, and this in spite of the date—1897—on the cover of the book.

We wish to enter an objection against the practice—very much to the fore in this article—of printing the same figure twice or three times, on each occasion with a new number and with no indication that it has

appeared previously. Here we see figures 38, 39, 40 and 41, repeated as figures 44, 45, 46 and 47, whilst the figure originally appearing as fig. 36, does duty no less than three times. No doubt it is convenient to have the drawing on the same page as the letterpress referring to it, but in the present instance, at any rate, we think that space might well have been economised, even at the sacrifice of convenience.

In the other sections things are scarcely more satisfactory. The description of indirect division of the nucleus is short, bald, and utterly insufficient, there is practically no discussion, and no mention of many most important points.

It is perhaps significant that the bibliography includes thirty-two references only, and that amongst these there does not occur the name of a single English or American author.

In the next important article—that on the Brain—an excellent account of most of the known facts is given, and in the space of nearly 500 pages a mass of information is collected and arranged in such a way as to form a most useful storehouse, to which constant reference will undoubtedly be made.

The article opens with a historical sketch of 120 pages, then follow seventy-four pages on General Morphology, and a section on the cerebral circulation.

The bibliography of this section seems particularly full and up to date, the titles of most of the important papers which appeared in 1896 being inserted.

The rest of the article on the general *rôle* of the cerebral cortex, the cortical centres, and the conducting functions of the brain, are all well and fully treated, but unfortunately the bibliography has been somewhat neglected, and this is the more important in a book which one would naturally turn to for a full and representative list of papers.

In the article on Heat again we have a fairly complete and exhaustive account of the subject, and the first section on Heat Production in living beings has a bibliography extending to nine pages. Then follows a short section on General Biology, and in section 3 we have an account of the action of heat on living beings, with a bibliography of twelve pages.

For these two important articles then we have nothing but praise;—they are worthy of the work in which they appear, and will prove useful to physiologists for many years to come. As regards the rest of the articles most of them are sufficiently fully treated, or at all events as fully as their importance justifies when space is limited—but we must repeat that in a work like the present, that although of course there cannot be included exhaustive accounts of every point in physiology, yet there ought certainly to be given in every case a full bibliography, and the editor would do well to insure such being provided in future sections.

Open Air Studies in Botany. By R. H. Praeger, B.A., B.E., M.R.I.A.
London: Chas. Griffin & Co., 1897.

The idea of this book is distinctly good. It is of course not quite original, having already been developed by Warming and others. The author's aim is to give an account of the characters of the flora of a locality regarded from the standpoint of the physical conditions there prevalent, and he has wisely refrained from attempts at too ambitious a treatment. Thus he selects types such as a meadow, an alpine, a bog flora, and treats of those constituent plants which may be found growing at a particular season. In fact the book forms a series of excursions, and as such it will be of use in indicating not only the general character of a physical flora, but also what time of year it may most advantageously be visited. A good feature of the volume lies in the photographs of plants in their natural surroundings, but the reproductions are not very successful in every case. The glossary is perhaps the weakest part of the book, many of the definitions as they stand, if not actually erroneous, are at least misleading. These defects ought to be remedied in a new edition if it should be called for. On the whole the book is useful and interesting, even where one does not agree with the writer's statement or opinions.

Lehrbuch der Botanik für Hochschulen. Von Dr. E. Strasburger, Dr. Fritz Noll, Dr. Heinrich Schenck, Dr. A. F. W. Schimper.
Dritte verbesserte Auflage, mit 617 z. th. farbigen Abbildungen.
Jena: Gustav Fischer, 1898.

Students of Botany will welcome the new edition of the "Bonn Text-book". That the book has achieved success is abundantly proved by its having passed into the third edition in less than four years, and we may add that its success is well deserved.

The original plan was excellently conceived and its execution has been materially improved in its present form. A few new illustrations meet the eye of the reader, whilst some of the old ones have been replaced by better ones. Amongst the latter, we notice that the centrospheres and centrosomes are now absent from the Phanerogam cell. The text also has been brought up to date, thus recognition is accorded to Williams' recent work on the Dictyotaceæ.

A bibliography at the end of the volume will prove a useful addition, though it might, perhaps, have been with advantage distributed through the various sections to which it respectively belongs. Altogether the authors and the publisher may be congratulated on having produced one of the best botanical text-books of its grade in existence.

A Student's Text-Book of Zoology. By Adam Sedgwick, M.A., F.R.S.
Vol. i. Pp. xii., 619. With 472 illustrations. London: Swan Sonnenschein & Co., 1898.

As Prof. Sedgwick states in his preface, this work is intended to replace his translation of Claus' *Lehrbuch*, and is consequently prepared upon the same lines.

This book is not intended for the elementary student, nor is it written as a practical laboratory guide, but it is thoroughly suited to the needs of the more advanced students in their second and third years, and to all those who have some general knowledge of zoology. And further, the inclusion of so large a number of genera, even though many of them are merely mentioned by name (the index covers twenty-seven pages, which gives some idea as to the great number of forms dealt with) makes this work an extremely useful book of reference to all interested in Natural History.

We are glad to see that Prof. Sedgwick has so far omitted the usual introductory chapters dealing with the principles of zoology, these in his translation of Claus' *Lehrbuch* occupied 181 pages, and consequently curtailed the systematic part of the book considerably. We hope Prof. Sedgwick will reconsider his determination to introduce this matter into his second volume, and that he will rather allow his second volume to follow on the lines of his first. When this is complete, let him by all means write us a book on the principles of zoology, for which, we believe, there is great need; but let it be a book all to itself, the subject being one that is too important to be treated, as is usually its fate, in a few chapters appended to a systematic text-book, which necessitates one or other of the parts being unduly curtailed.

One short introductory chapter, however, appears to us to be needful, setting forth Prof. Sedgwick's views upon the cell theory. Our author appears to imagine that all students are brought up to believe in the views on this subject which he has so ably maintained in various papers published in the *Quarterly Journal of Microscopical Science*, and that these views are universally accepted. Hence in his introduction to the Protozoa and Metazoa, he most carefully avoids all mention of the word cell, a state of affairs which, we are afraid, will tend to perplex a great many students, whereas a brief statement of his views on this subject would have saved the student much unnecessary worry besides bringing clearly before him a very important interpretation of the animal body, which has no doubt in the past been obscured by a too rigid adherence to the cell theory of the older zoologists.

It is, of course, impossible for one man to be a specialist on every group of animals, and Prof. Sedgwick is to be congratulated on having secured the co-operation of so many able zoologists—to whom he gives due credit in his preface—to assist him in revising special parts of this work.

To deal with a few points in detail, we could wish that a little more space had been devoted to the Calcareo among the sponges, as this group so beautifully illustrates the possible lines of evolution of the complex sponge organisation, and especially that a few more figures of the anatomy of these forms had been given, such, for instance, as *Leucoselenia*, etc., with some reference to Minchin's admirable work on this group.

The book would be improved by the addition of a few more illus-

trations, especially of anatomical points bearing upon the classification of the orders. For example, we find Bürger's classification of the Nemerteans adopted, which, as is well known, is largely based upon the relations of the nervous system to the muscular layers of the body wall, but not a single figure is given in illustration of this important point.

Prof. Sedgwick justly lays great importance upon the presence of a true coelom, but there are certain disadvantages in the present state of our knowledge attending the adoption of the coelomata as a group of the Metazoa, for it causes a complete and wide separation of the Rotifera from the Annelida, whereas judging from the striking structural resemblance of certain of the former with the trochosphere larva of the latter group, one would have been inclined to place them close together, and when we remember that the development of the sexually produced embryo of the Rotifera is still unknown, and consequently also the true origin of its body cavity, it seems somewhat premature to so widely separate these two groups. The fact that in the adult Rotifera and Nematoda neither the excretory nor the generative organs appear to be connected with the body cavity, does not entirely preclude the possibility of the latter being a true coelom, therefore it appears to us that Prof. Sedgwick's adoption of the group coelomata, however prophetic it may be, is nevertheless distinctly premature and inadvisable in a student's text-book.

We do not think that the majority of molluscan specialists will agree with our author in his severance of the Chitons from the Solenogastres, and to state that such an association was "quite unjustifiable" and of the latter that "they are not Gasteropoda," when we consider the eminent malacologists who have grouped them together, appears to us to be hardly supported by sufficient evidence, and Prof. Sedgwick goes further and throws out a suggestion that the Solenogastres may not be mollusca at all. The main reason given being that the gonads in the Solenogastres open directly into the pericardium, but, as Prof. Sedgwick points out in other parts of this book, the latter is only part of the coelom in connection with which the gonads are invariably developed, and with which the perigonadial cavity communicates through the kidney (another part of the coelom) in *Fissurella*, *Haliotis*, some Lamellibranchs and more directly in the Cephalopoda, and when we consider the similarity of the nervous system of the Solenogastres with that of Chiton, the condition of the foot in *Cryptochiton* and that of the organ believed to be its homologue in *Neomenia*, and the many other points of resemblance, I think the majority of us would conclude that the group Amphineura was a natural one, and that at all events the Solenogastres were mollusca, and found their nearest relatives in the Polyplacophora. A few more anatomical illustrations might well have been accorded to this group, and a reference given to Simroth's fine work on these forms in Bronn's *Thierreich.*, Bd. 3., Abth. 1.

The molluscan part of this work is by no means the most satis

factory, and we hope in the next edition of this work to see it revised by a specialist. Prof. Sedgwick has adopted many of Pelseneer's beautiful figures, but he has retained one or two inferior ones, notably fig 282, the nervous system of *Paludina* after v. Jhering, in which the otocysts are represented as innervated from the pedal ganglion which, as our author must be aware from his statement on p. 367, is a mistake, and one which any one can verify in a moment by dissecting a *Paludina*, or by referring to Bouvier's great work on the nervous system of the Gasteropoda (*Ann. Sci. Nat.* (3) iv., 1887), which might well have been quoted here. In connection with the Mollusca, we should like to draw Prof. Sedgwick's attention to a small mistake on p. 336, where in speaking of the relation of the heart and rectum in certain Lamellibranchs he states exactly the reverse of what he meant to say, the rectum in *Ostrea* being dorsal to the heart and in *Arca*, etc., ventral, and not the reverse as stated.

When one considers the enormous work involved in writing a book of this description, one marvels at the accuracy and freedom from mistakes which this text-book exhibits, it should prove of great service to all students of zoology. We must congratulate Prof. Sedgwick heartily on the appearance of this volume, and hope soon to welcome vol. ii.

Lehrbuch der Entwicklungsgeschichte des Menschen. Von Dr. J. Kollmann, o.ö. Professor der Anatomie in Basel. 8vo, pp. xii., 658. With 386 figs. Jena: Gustav Fischer, 1898. Price 15 marks.

When we consider the somewhat limited material from which a student of Human Embryology can draw his knowledge of this subject, we must congratulate Dr. Kollmann upon the excellent work which he has produced. But we fail to see the need of these large works upon Human Embryology; a short work suited to the needs of a medical student we could appreciate, but large books like the present or such a work as Minot's *Human Embryology*, if they really dealt with the human subject alone, can fill no real void. It is the title of these works, however, that is at fault, they are in reality text-books of vertebrate embryology, making a speciality of the mammalia and including most of what is known concerning the development of the human subject. And this must necessarily be the case, for no matter what organ or what developmental point of real significance we choose to study, it would be impossible in the present state of our knowledge of Human Embryology, and possibly always so, to understand the importance or the real meaning of the facts which we might observe, without a thorough knowledge of the Comparative Embryology of the vertebrata generally.

The present work, while introducing a considerable amount of comparative embryology, especially in dealing with the formation of the germ-layers and of the first rudiments of the organs, restricts itself as far as possible to the human subject and deals very fully with the

building-up of the human organs and traces them to their definite form. Especially excellent in this respect are the sections dealing with the blood vessels, the urinary and genital organs; in the latter we find the recent work of Keibel given due prominence. That, however, dealing with the development of the genital products is not so good, the formation of the spermatozoa being treated with undue brevity and its comparison with the maturation of the ovum being overlooked.

We should have thought that some of Heape's work on the germ-layers in the Mole might have been included with advantage in this book, and surely also the student of human embryology should know something about the condition of the ovum in the Monotremata which throws so much light on the peculiar processes met with in the segmentation of the ovum of the Placentalia.

A large series of beautiful process blocks, many of which are coloured, illustrate this work, and among them we find many new and useful figures, especially in the more anatomical part of the book.

The book should be of great use to the student of human anatomy and embryology, but it is too specialised and lacks the comparisons necessary to any one making a general study of vertebrate embryology.

The Romanes Lecture, 1898: *Types of Scenery and Their Influence on Literature*. By Sir Archibald Geikie, D.C.L., F.R.S. London: Macmillan & Co., 1898.

The question treated in the Romanes lecture for the current year is one possessed of a more directly human interest than is the case with many scientific discussions. Sir Archibald Geikie traces, with necessary brevity, the influence of scenery on the minds of some of our great writers. None can doubt the reality of such an influence on all persons with a spark of imagination in their composition, but it is interesting to find it carefully followed and proved in the case of writers like Cowper as the type of the English lowlands and of Burns as representing the Scotch lowlands. The author classifies the characters of scenery into Lowland, Upland and Highland. In his treatment of the last-named group he adduces the poems of Ossian for Scotland and of Wordsworth for North-west England. If it is not possible to criticise in detail a lecture into which, as Sir Archibald himself says, subject matter for a whole course has had to be compressed, it is needless to add that it will be found full of interest for the general reader, and that merely as a study of English composition and style it will well repay a perusal.

APPENDIX.

NOTICES OF BOOKS.

The Tailless Batrachians of Europe. Part ii. By G. A. Boulenger, F.R.S. London: The Ray Society, 1898.

The present year will be memorable in the popularisation of Zoology, if only for the appearance of this splendid and inspiring treatise, now complete. The part before us, paged serially with its fellow, deals on the same lines as that with the Bufonidæ, Hylidæ, and Ranidæ, and there are given in illustrations fourteen plates, four maps, and forty-four text figures, all of which are worthy the text and the preceding part which has everywhere been hailed with acclamation. Concerning the general merits of the work, we have nothing to retract from our wholly laudatory comments upon the first part. All that is important in taxonomy and distribution, and of interest concerning breeding habit and racial varieties, has, up to the requirements of the work, been collected together and presented to the reader in a form well calculated to arouse his enthusiasm and interest in the familiar though much despised Frogs and Toads. And for the English student the book has an especial value, in being based upon the matchless collections preserved in our own Natural History Museums, a full list of which is appended. It teems with originality, and one of its most conspicuous features is the recognition of observations on habit and geographical limitation made in the field of the self-trained nature-loving traveller, who, in our present severely academic days, is apt to be considered unscientific, and thereby discouraged in what the perusal of the pages of this book amply proves to be good and useful work. It is for such as these that the book and its companion volume are primarily intended and most heartily do we congratulate both the author and the Ray Society upon their completion. In publishing them they have set up a high standard, and we would reiterate the hope that they will with as little delay as possible follow up this their new venture, and provide correspondingly for the treatment of the Tailed Batrachians, which in many points of popular interest and scientific value even excel their tailless allies.

Die Zelle und die Gewebe. Grundzüge der Allgemeinen Anatomie und Physiologie. Zweites Buch. Allgemeine Anatomie und Physiologie der Gewebe. Von Prof. Dr. Oscar Hertwig. Jena: Gustav Fischer, 1898.

Every one who has read the first volume of Prof. Hertwig's *Zelle und Gewebe* will open the second part with an assured feeling

that he is going to be interested. And the perusal of the book will not disappoint the reader's expectation.

The author discusses the nature and the complex interaction of the causes which operate in effecting the development and the diversity of organic forms, and thus at once brings us face to face with problems no less interesting to the morphologist than to the physiologist. Opening with an account of the morphological and histological peculiarities of tissues Dr. Hertwig proceeds to examine them from the point of view of the functions they severally discharge. He criticises acutely the reasoning of writers who are ready to refer all the phenomena of life to processes analogous to those exhibited by a machine, and points out some of the insidious fallacies which underlie such comparisons.

He insists strenuously on the fact that there are two distinct factors, both of which modify the course followed by the constituent cells of a developing organism; first, the *external*, and second, the *internal* factors. And he well brings out the point so vitally important for his own theory of organised development, namely, that the very presence of a number of cells in a multicellular individual must cause a reaction amongst themselves, since each one is environed by all the rest. Of course this was pointed out by Herbert Spencer many years ago, but it has not always received the attention it deserves, and in fact it is deliberately ignored in one of the most carefully worked out of the current theories of heredity. It may be remarked in passing, that Prof. Hertwig might have considerably strengthened his case for the actively modifying effect of the environment by a reference to Klebs' recent work on algæ and fungi, in which the various factors which determine this or that course of development have been successfully distinguished and isolated.

The *internal factor*, however, it must be confessed, still remains as great a puzzle as ever. We are really totally ignorant of the nature of the causes which determine the development of this or that animal or plant out of apparently similar primordia. Nor are we possessed of any definite knowledge as to the nature of the processes which we term *correlative*. Of course it is clear that a correlation between the various constituent cells of an organism must and does exist, but beyond this we know very little. In the animal world, the experiments of Driesch, Wilson and others on embryos have considerably broadened the basis of facts, but, without in the least desiring to detract from their importance, they cannot be said to have done much more. It is, moreover, not unlikely that the production of "identical twins" in the human species may be due to the separation and independent development of the halves of a fertilised ovum which normally would have developed into one individual, the two halves sinking their separate individuality in producing the single child.

In the vegetable kingdom the zygotes of some fungi and algæ may either grow out directly into single embryos, or they may give rise to several embryos apiece if the first formed cells separate from one another

and become independent—that is to say, instead of a common and correlative development giving rise to one organism, an individualistic segregation occurs amongst the first-formed cells and produces a number of separate plants—and analogous cases are met with even amongst gymnosperms. What nature has done in these plants man, as already stated, can accomplish by the employment of special means, in the case of some animals, though of course the results are not so perfect. But in principle they are probably not dissimilar.

To return to Dr. Hertwig's book, we find that he admits, as essential to his theory of Biogenesis, the existence of two substances which together make up the living substance. The first, the hereditary and governing substance, he calls Idioplasm, following in Nägeli's footsteps, whilst he also postulates the existence of a "Formed matter" (Beale) or *Protoplasma-product*. The latter is moulded by the idioplasm, and is caused by it to assume the particular character distinctive of the various cells and tissues of the body.

But this separation or distinction of the living substance into idioplasm and formed plasm is, under various guises, common to most of the more recent theories put forward to explain the *εἶναι* and *γίγνεσθαι* of organic life, nor do we find anything strikingly novel in the notions that not only is the idioplasm the substance conserving the hereditary qualities of the species, but that also it is capable of receiving impressions from without, and thus, itself being altered, modifying its action as the formative material of the cell. Indeed, Dr. Hertwig expressly admits the possibility of the inheritance of acquired character, but regards such inheritance as an important fact with which his theory of Biogenesis is competent to reckon. Nevertheless, the examples he quotes are singularly unconvincing. That "attenuated" bacteria may reproduce attenuated forms is really not to the purpose, since there is no sexual propagation in these forms, but merely an increase of cells resulting from vegetative activity. One might as well attempt to base an argument as to the inheritance of acquired characters on the behaviour of a rose-bush manured in one year and starved in the next. The rose-bush *cells* will, it is true, not be the same in the successive seasons, but the different results in the case of the new starved tissues have no sort of bearing on the inheritance or non-inheritance of acquired characters in the sense in which these are ordinarily defined and understood. And the different bacterial generations are really analogous to the new generations of the rose-bush (somatic) cells.

And the other instance quoted by the author in support of the transmission of acquired characters seems, to the present writer, rather to tell against the assumption that the *hereditary substance*—the idioplasm—was in any way really affected. This instance referred to is derived from the researches of Ehrlich upon the action of Ricin on mice. This investigator found that it was possible to immunise these animals by slowly rendering them accustomed to the poison, and, further, that this immunisation could be partially transmitted to the

offspring. Hertwig (drawing his own conclusions, which differ from those of Ehrlich) seizes on the latter point and regards it as an example of inheritance of acquired characters, but it would seem that it is possible to explain the facts quite differently. For it is only the *female* which can transmit the immunity, and even then it only persists for a limited period of the early life of the offspring. Now it is curious that the male has no such power, but it should be remembered that the sperm consists almost entirely of nuclear idioplasmic matter. The egg, on the other hand, contains in addition a relatively enormous mass of non-idioplasmic protoplasm, and it seems likely enough that, just as the protoplasm of the body cells of the parent was rendered immune, so also this bulky substance in the egg possessed a similar property—possibly by virtue of the possession of some antitoxin, or of some antecedent of an antitoxin; and this might readily explain why the immunity soon passed away from the growing offspring. At any rate the evidence for the idioplasm having become itself affected seems extremely precarious and of doubtful value.

This case has been discussed rather fully as it is just one of those instances in which the meaning of “inheritance of acquired characters” may be rendered obscure or ambiguous through not clearly emphasising the difference between an influence affecting the idioplasm and one merely restricted to the non-idioplasmic but still living protoplasm.

We could wish, indeed, that the theory of Biogenesis itself had been set forth somewhat less vaguely; it is difficult even to state definitely the main points of the theses embodied in it, but its key-note lies in the idea that the idioplasm is capable of receiving and becoming modified by impressions from without, and that, once such an impress has become one of the characters of the idioplasm of a species, a bias will be manifested which will tend to direct further development along certain definite lines, and thus the course of the future evolution of any given species is profoundly influenced by the steps which, in its ancestral line, it has already traversed.

There is much in Dr. Hertwig's theory which reminds the reader of Nägeli and Lamarck, and, indeed, the author expressly mentions his obligations to these, as well as to many other predecessors, but the original method of treatment, and the many luminous passages with which it is adorned, will ensure a welcome for the book even at the hands of those who are unable to accept the views which it is its chief purpose to express.

J. B. F.

Fossil Plants ; for Students of Botany and Geology. Vol. i. By A. C. Seward, M.A., F.G.S., Lecturer in Botany in the University of Cambridge. Pp. 452 ; with 112 Illustrations. “Cambridge Natural Science Manuals.” University Press. 1898.

The need of a trustworthy and readable manual of Fossil Botany has been urgently felt for some time. Within the last few years

botanists generally have begun to wake up to the importance of the subject, but there has been no suitable book to put into the hands of students, and even the teachers have scarcely known what to teach. It is probable that the same want has also been experienced on the geological side.

By far the best general hand-book has hitherto been Count Solms-Laubach's *Fossil Botany*, translated into English for the Clarendon Press, a work which remains indispensable for the advanced student, but which is scarcely suited to rouse the interest of a beginner, and now much needs bringing up to date.

Mr. Seward's book—judging from the first volume—will fill up this gap in scientific literature in a most satisfactory way. He has had a good opportunity and has known how to make use of it. The manual is interesting, accurate and critical; the last quality, above all, is essential to any useful treatment of Fossil Botany. It is safe to say that a large part of the contents of this volume will be absolutely new to all except the few who have kept up with the special literature of Fossil Plants. Apart from the particular subject, the appearance of a thoroughly good and original English text-book on an important branch of botanical science is in itself a matter for congratulation.

The present volume includes a general introduction, occupying six chapters, and the first instalment of the special systematic work, embracing the Thallophyta, Bryophyta and two divisions of Pteridophyta.

The first chapter of the general portion is devoted to a short history of Fossil Botany, interesting enough to make one wish it longer. The author is sympathetic and appreciative in his treatment of the older writers, but shrewd as their judgment often was we cannot help feeling that scientific Fossil Botany really sprang into existence with the advent of the great Brongniart.

A useful chapter follows on the relation of Palæobotany to Botany and Geology. It is to be wished that the caution conveyed in the following words were more generally observed: "In working out any collection of fossil plants, it would be well, therefore, to bear in mind that our aim should be rather to reproduce an accurate fragment of botanical history than to perform feats of determination with hopelessly inadequate specimens. Had this principle been generally followed the number of fossil plant species would be enormously reduced, but the value of the records would be considerably raised" (p. 16).

The third chapter is on geological history. A manual of fossil plants cannot be expected to teach Geology; but the summary given here, with its useful tables of strata, will be of much service to the botanical student. The account of the divisions of the all-important Carboniferous Formation, and of their distribution, strikes us as particularly good.

The general chapters are provided with mottoes—apt and sometimes amusing, especially the quotation from Pope which precedes chapter iv., on the preservation of plants as fossils:—

“ The things, we know, are neither rich nor rare,
But wonder how the devil they got there.”

This chapter contains, among other things, some interesting details of the processes of silicification and calcification on which the preservation of vegetable remains with structure depends. The author justly lays stress on the extraordinary perfection with which histological structure is often preserved, a point never realised except by those who have themselves examined good specimens. Mr. Seward is convinced that in certain favourable cases even the nuclei of the cells are to be recognised.

Chapter v., on difficulties and sources of error, is very salutary, though “ the description of a fragment of the handle of a wedgewood teapot as a curious form of Calamite ” (p. 94) was no doubt an extreme case ! The author justly insists on the uncertainty of characters drawn from the form and venation of leaves. In fact, external characters generally, when unsupported by evidence from structure, are only to be used with the greatest caution.

Chapter vi., on nomenclature, is commendably short, and free from pedantry.

This brings us to the main part of the book, the systematic exposition of the families of fossil plants. This is very fully done, and gives proof of much research, and of sound criticism, based on personal examination of the actual specimens.

Mr. Seward’s remarks on fossil Bacteriology, a subject of which a good deal has been heard lately, are much to the point. We quote one sentence : “ Specialists tell us that the accurate determination of species of recent Bacteria is practically hopeless ; may we not reasonably conclude that the attempt to specifically characterise fossil forms is absolutely hopeless ? ” (p. 138).

Among the Algæ the author calls attention to the valuable evidence for the geological history of the Calcareous Siphonæ, which present a marked contrast to other so-called fossil Algæ, most of which are not Algæ at all.

As regards the Bryophyta the fossil evidence is still almost entirely negative.

It is when we reach the Pteridophyta that the serious interest of fossil remains for the botanist begins. Among the Vascular Cryptogams and Gymnosperms it is not too much to say that the value of the fossil evidence to the comparative morphologist equals that from the recent plants themselves.

The botanical student will at once be struck by the presence of *four* instead of three main divisions of Pteridophyta. The fourth group—Sphenophyllales—is entirely fossil, and in fact only known at present from the Palæozoic rocks.

Among the Equisetales the structure of the recent genus is first described (no doubt for the benefit of geological students), and then the fossil forms are taken in order, beginning with those nearest the living

representatives. The author points out that many of the Calamites had a leaf-sheath, so that this supposed distinction from *Equisetites* falls to the ground.

A thoroughly trustworthy account of Calamarian anatomy is given, in which the common English type *Arthropitys* is treated in the fullest detail. A good new figure is given here, showing the connection between the roots—formerly named *Astromyelon*—and the stem.

The cones are fully described; those with structure preserved, both homosporous and heterosporous are first dealt with, and then, in the light of these, the more doubtful specimens showing external characters only are discussed.

In the treatment of the macroscopic characters of the stem, the provisional genera of Weiss are adopted.

The chapter finishes with an account of the interesting genus *Archæocalamites*, the most ancient member of the family known to us.

The last chapter in the volume, that on the Sphenophyllales, is perhaps the most valuable in the book, for it gives for the first time a coherent and correct account of the whole organisation of these remarkable plants. Previous descriptions, even in the best hand-books, are all either untrustworthy or defective, and until now it has only been by the careful study of original papers that a true idea of the family could be built up. The author's suggestion that these graceful plants may have had a climbing habit is original and worthy of consideration. He adopts the view that this extinct phylum combines Equisetaceous with Lycopodiaceous affinities.

The book, excellent as it is, is not of course free from errors; the most startling perhaps, though an obvious slip, is the inclusion of *Chondrus crispus* among *Phæophyceæ*! The manual was so urgently needed, and fulfils its purpose so well, that we hope there may soon be an opportunity of correcting such slight defects in a second edition.

The illustrations are thoroughly good and business-like. The greater number we owe to the skilled pencil of Mrs. Seward. The second volume will be awaited with considerable interest.

D. H. S.

Lectures on the Theory and Practice of Vaccination. By Robert Cory, M.A., M.D. Cantab., F.R.C.P. Lond. London: Baillière, Tindall & Cox. 1898.

Dr. Cory has for twenty-two years been engaged in the practice and teaching of vaccination, and here in a series of six lectures he has given to the world some account of the results of his unique experience. Lecture I. deals with the reasons which led the legislature of this country to impose the vaccination laws upon the people. It is unfortunately impossible for the opponents of vaccination at the present day to see examples of the horrors which the "hated rite" is calculated to prevent; for as a direct result of the widespread adoption of this most valuable preventive measure small-pox is so much less frequent

than it previously was that the opportunities of studying it in England are now far from numerous and even when they do occur the disease is usually deprived of most of its worst features.

The mere fact that before vaccination was introduced it was an everyday occurrence to meet in the streets numbers of people bearing all the disfigurement of small-pox pitting, whilst now it is quite an exceptional circumstance to see such a case, is sufficient evidence of what vaccination has done. But the very absence of such examples tends to make one forget the horrors of small-pox and to tolerate senseless agitators who attempt to convert others to their own absurd theories.

To form a due estimate of the matter one must be in a position to judge on the one hand of the evils of the disease to be exterminated and, on the other, of the advantages claimed for the means proposed for the purpose. In the present instance we are mercifully deprived of the former and it is perhaps, therefore, better to take the opinion of one who lived at a time shortly after the discovery of vaccination and who was therefore in a position to estimate the evils of small-pox and the advantages of vaccination.

Mr. Cross, writing in 1820 (twenty-two years after the discovery of vaccination) of the epidemic of small-pox which visited Norwich in 1819, says in conclusion: "I advocate vaccination because I believe it to be the most powerful means of preventing the misery attendant on disease and of saving human life which Providence has vouchsafed to put into the hands of man; my time has been given up to the gratuitous practice of it, because I can thus do more good amongst the poor than by prescribing pills and potions, and I regard every drop of the vaccine ichor as the most active material that can be admitted into the list of our prophylactic remedies. I am therefore grateful to the philosopher who has taught us to wield this weapon of defence in overcoming *the worst of human maladies*."

And this is the unbiassed opinion of a man fully capable of forming a sound judgment and having the knowledge born of experience of small-pox unmodified by vaccinia and of the improvements which followed on the introduction of the inoculation advocated by Jenner.

Dr. Cory has had the good sense to give his information in a tabular form wherever possible. Thus the tables on pp. 12-33 show clearly the influence of vaccination in delaying the average age at which small-pox makes its attacks, and further the additional security conferred by several incisions as compared with one only. For instance, in those cases where the patient was admittedly unvaccinated the average age at which small-pox occurred was 6.58 years. In those stated to have been vaccinated, but having no scars, 9.86 years; in those having one scar, 17.77 years; two scars, 17.82 years, and in those having five or more scars, 19.3 years.

The table opposite page 6 is also very instructive. It shows by means of vertical lines the average yearly deaths from small-pox per 100,000 inhabitants in Prussia, with compulsory vaccination and

compulsory revaccination at the age of twelve years ; Holland, with compulsory vaccination of children before entering a school, and of Austria, without compulsory vaccination. In the first case, that of Prussia with compulsory vaccination and compulsory revaccination at the age of twelve years, the vertical line representing the number of cases of small-pox is nearly $3\frac{1}{2}$ inches long and indicates about ninety cases for the years immediately preceding the passing of the vaccination law, whilst the same line subsequently to the passing of the law in no year projects more than about one-eighth of an inch above the base line.

In the case of Holland, as might be expected from the absence of compulsory revaccination, things are rather less satisfactory. Before the passing of the law the vertical line is again about $3\frac{1}{2}$ inches long and represents about ninety cases per 100,000 of the inhabitants, whilst after the law was passed for several years the line remained very short ; but in 1883 it was rather over $\frac{1}{2}$ inch and represented about fifteen cases. In Austria, where there is no compulsory vaccination the average yearly deaths from small-pox per 100,000 of the inhabitants in 1868-1874 were represented by a line $4\frac{3}{4}$ inches long (about 120 cases) and since that time the line has never been less than $1\frac{1}{2}$ inches, it has in one year been $3\frac{1}{4}$ inches and in another $3\frac{3}{4}$, representing about eighty-two and ninety-seven cases respectively.

It is difficult to give by means of mere figures any idea of the effect produced on the mind by a table of this kind which bring forcibly before one the actual facts of the case. The extraordinary diminution in the number of deaths from small-pox in those countries adopting vaccination, contrasted with the continued high mortality in countries where such precautions are neglected, shows very vividly the immense advantages to be obtained from a proper use of the means ready to our hands and no one with an unprejudiced mind who cares to read this chapter can fail to appreciate the value of vaccination as a preventive of small-pox.

In the second Lecture Dr. Cory gives an account of the histology of the vaccine and small-pox vesicles. The account is illustrated by drawings and photographs, and Dr. Cory incidentally shows how different is the structure of the syphilitic Chancre, such demonstration seeming desirable from the fact of the statement having been made that every vaccination vesicle is a lesion of a syphilitic nature.

The third Lecture is devoted to a consideration of the differences between a primary and a secondary vaccination. By a series of tables and figures Dr. Cory has been able to place the matter in a clear light, and on the interesting subject of how far vaccination can be regarded as of use in cases where patients have already been exposed to infection he shows that vaccination to be of use must be performed at all events within the first four days after such exposure.

The eruptions that occasionally follow vaccination are treated in Lecture IV., and this important section has been illustrated by means

of coloured drawings of the more frequently seen affections. Dr. Cory's reasoning on the vexed question of how far these eruptions are directly due to vaccination is close and accurate. Moreover his observations are marked by sound common-sense and knowledge of his subject, and we cannot do better than recommend all who seek information on this point to refer to his work.

The practical details of vaccination occupy Lecture V. It goes without saying that the experience of a man like the author, who has had such ample opportunities of observation and such wide acquaintance with the practical work of vaccination, who, moreover, is such a past-master in all essential details that his success for the past twenty-two years has averaged 96·18 per cent. is invaluable, and specially interesting is it to have the results of his experience of the much-discussed question of insusceptibility. Among 16,000 first attempts of vaccination with humanised lymph Dr. Cory has had fourteen failures, being at the rate of one failure in 1140 children operated upon.

Amongst 21,781 children vaccinated on the arm with calf lymph he has experienced seventy failures at a first attempt, being at the rate of one failure in 311 children operated on. From the above figures and certain calculations, which it is unnecessary to enter into here, it has been determined that one case of statutory insusceptibility as evidenced by failure to obtain any vaccine vesicle in a given child after three several attempts at vaccination, may be looked for in every 6,684,495 cases when working with calf lymph, whilst with humanised lymph the figures stand at one in 329,232,000, so that there is not any great probability of numerous cases of insusceptibility occurring in the practice of a country vaccination!

The statistics given as to the keeping qualities of calf and human lymph on points and in tubes are of utility, and of especial importance are the author's observations and clever illustrations of the relative effects of vaccinating in few or many places.

We fancy there is a plate missing somewhere, as we have failed to discover the promised figure of the vaccinated calf, method of applying forceps, etc. The chapter concludes with a tabulated statement of the symptoms observed in a case of vaccination from the first to the twenty-first day, and some observations on secondary vaccinations.

The last Lecture is devoted to a discussion of the relations of cow-pox, horse-pox, and camel-pox to small-pox, and Dr. Cory comes to the conclusion that vaccinia is but a modified form of small-pox, and that small-pox is the origin of cow-pox, horse-pox and camel-pox.

Throughout the 122 pages of which the work consists the author's statements and conclusions are characterised by an evidence of wide experience, keen insight and sound common-sense, and our only regret is that with such extensive knowledge and practical experience Dr. Cory has been content to publish so small a volume. We hope that in a second edition he may see fit to enlarge it considerably.

Essays on Museums and other Subjects connected with Natural History.

By Sir W. H. Flower, K.C.B., F.R.S. London: Macmillan & Co., Ltd., 1898.

This elegant volume contains a reprint of various addresses, lectures and obituary notices, delivered and written by their author within the past twenty-eight years—the earliest (1870) being his Introductory Lecture as Hunterian Professor of Comparative Anatomy to the Royal College of Surgeons, the latest his article on “Boys’ Museums,” which appeared in *Chambers’ Edinburgh Journal* for April, 1897. The contents of the book are arranged under four sections, *viz.*: one of 96 pp. dealing with “Museums,” the next of 138 pp. with “General Biology,” a third of 122 pp. devoted to “Anthropology,” and a fourth of 34 pp. embracing some “Biographical Sketches,” making a total of 390 pp. in all.

The author’s real work in science and scientific education, of which so many of these essays are the popular outcome, now stands monumental in our two greatest London Biological Museums, and the views which the essays contain are sufficiently well known and have been already sufficiently adopted and criticised to render it needless here to comment upon them in detail. A few trivial alterations necessary to bring this work up to date, and a preface, alone constitute the novel portions of the book; and of these the latter bears a melancholy strain in the declaration that the decision to publish the work has been the outcome of “an enforced period of restraint from active occupation”. By his charming personality and the adoption of ever peaceable and conciliatory methods, the author, friend of Darwin and Huxley, a working naturalist whose “scientific life began before the publication of the ‘Origin of Species,’ ” has won for himself a popularity and a deservedly representative position among English-speaking zoologists, and has been honoured in the awards of the highest continental distinctions. Respect for the work and views of his contemporaries and abhorrence of the aggressive appear to have been the guiding principles in his action, and to those who know him the present volume will remain a lasting token of affection and goodwill. Throughout its pages we meet with due recognition of the help afforded him by others, and we accordingly regret the absence of his Royal Institution Lecture of 1886 on the “Wings of Birds,” in which there was first made public the memorable rediscovery of “aquincubity,” so lamentably associated with the late Richard Wray, than whom Sir W. Flower has had no more ardent admirer nor loyal lieutenant in the later and crowning labours of his life. We cannot allow the opportunity to pass of congratulating the publishers upon the choice and get up of the cover of this work, so strongly in contrast with the familiar green cover of their scientific text-books.

Practical Plant Physiology. An Introduction to Original Research for Students and Teachers of Natural Science, Medicine, Agriculture and Forestry. By Dr. W. Detmer, Professor of Botany in the University of Jena. Translated from the second German edition by S. A. Moor, M.A., Principal of the Gorasia College, India. London: Swan Sonnenschein & Co. New York: The Macmillan Co., 1898.

Those who have used Prof. Detmer's book will be glad to see an English translation which will render it available for the many students who are unable to read German. It must be confessed that the translation itself is not very attractive, and although this does not perhaps affect seriously the value of the book as a working guide, it greatly detracts from the pleasure of reading it.

It is not without some feeling of melancholy that one reads that the book is intended for those (amongst others) interested in Medicine, Agriculture and Forestry. Most of those who devote themselves to the two last-named pursuits in this country would consider such a course to be a pure waste of time; and with the modern contraction of the liberal scientific education which used to form part of the medical curriculum, it is to be feared that but few students in this profession will have much use for the book. But for those who are really interested in Physiology in the widest sense, Dr. Detmer's book provides a good introduction in so far as its relations to plant life are concerned.

Algemeine Physiologie, Ein Grundriss der Lehre von Leben, von Max Verworn, a. o. Professor an der Med. Facultät der Universität Jena. Zweite neu bearbeitete Auflage, mit 285 Abbildungen. Jena: Gustav Fischer, 1897.

To many people the appearance of a new edition, enlarged and improved, of Verworn's *Algemeine Physiologie* will come as a welcome addition to physiological literature, and even those who do not sympathise with the author's views will admit the interesting nature of the book. Of course in a work embracing so wide a field as the present volume of about 600 pages, the method of treatment is necessarily somewhat sketchy and unequal, but in spite of this the large amount of original work and of suggestive points of view will certainly assure for it a welcome at the hands of those who are interested in the physiological phenomena of living protoplasm.

Organographie der Pflanzen, insbesondere der Archegoniaten und Samenpflanzen. Von Dr. K. Goebel, Professor an der Universität München, Erster Theil. Algemein Organographie, mit 130 Abbildungen im Text. Jena: Gustav Fischer, 1898.

Prof. Goebel's book is one which should appeal to a wide circle of readers, for it provides a clear and connected exposition of the new vegetable morphology, in which the old formal treatment is almost

abandoned, to be replaced by one which seeks to unveil the causes which have operated on the plastic vegetable protoplasm and have resulted in the production of the manifold forms of the plant world around us. The writer is peculiarly well fitted for his task in-as-much as he has been himself one of the foremost investigators in this field. Prof. Goebel holds strongly that metamorphosis of a primitive structure is due to a change of function and hence it is useless to attempt to frame a scientific morphology without paying due regard to physiological circumstances.

The chapter on concrescence and abortion is very interesting as a summary of striking examples illustrating the author's standpoint.

Differentiation of form might however be conceivably due to an initial change taking place in the essential protoplasmic element—idioplasm or whatever else we call it—which results in a *diverse* response to a similar environment, and then selection would operate in eliminating the unfit and the less fit, so that finally only those forms would survive which were capable of suitably responding to the exigencies of their surroundings.

The sections treating of relations of symmetry and of the variations in the development of organs at different periods are for the most part based on the previous writings of the author, but nevertheless they have gained much from the way in which they are handled in their present form.

It is however surprising to find the old hypothesis of "*stoff und form*" revived in its crudest form on page 38. This hypothesis merely restates the problems to be solved in terms of substances as to the nature and even the existence of which we are entirely ignorant, and it only obscures the whole matter instead of clearing it up, to conjure with such figments of the imagination. But apart from this and a few other passages the book is an admirable one, and it would be ungracious to find too much fault on these grounds with a book so full of interesting matter that one does not want to lay it down till the last page is reached.

Outlines of Vertebrate Palæontology, for Students of Zoology. By A. Smith Woodward. Cambridge: University Press, 1898. (Cambridge Natural Science Manuals, Biological Series.)

This volume of 459 pages, with over 200 admirable illustrations, is most welcome, and we congratulate both the author and editors upon it. Palæontology, and that more especially of the vertebrata, has of late years been sorely neglected by a large section of our younger teachers and students. The intensity of comparative embryological research and a far too predominant pursuit of the "Schnitt Serie" method, together with other modes of investigation which favour broad generalisation, have almost blinded them to the more solid and tangible truths which lie buried in the rocks. And this movement has gone so far that there have not been wanting those who would not even admit it a branch

of Morphology, restricting that term solely to the study of the yet living forms. When, however, we turn to the record of progress, and consider the more revolutionary of our classificatory schemes, which after all are but popular expression of the growth of knowledge, it must be admitted that the influence of palæontology is paramount, and would seem likely to continue so. In their direct bearing upon our interest in living organisms, what more luminous than the discoveries of Seeley and Andrews on the Plesiosaurian limb girdles (so intuitively expressed in the generic name *Cryptoclidus*) as helping us towards a better conception of the affinities of the anomalous Chelonia; or the wonderful series of Permian fossils forming the "Eotetrapoda" of Credner, as showing that after all Hatteria, as affecting our conceptions of the inter-relationships of the lower terrestrial vertebrata, is perhaps the most wonderful and structurally central of those extant; or again, the unearthing in the United States of better and more complete remains of the Ancylopoda, as affecting our time-honoured and most cherished conceptions of correlation in tooth and limb structure among mammals? Palæontological discoveries such as these, like those of camels having a full series of incisors, of annectant giraffine forms, and the like, have not been lost sight of by those who take a broad survey of progress; and, considered alone, they serve as examples of palæontological study, having a far-reaching influence on our ideas of inter-relationship and structural affinity which transcends those of most purely embryological investigations, and justify the assertion that palæontology is not only a branch of morphology, but a foremost one, perhaps more forcible than the rest, and that the promise given in the earlier discovery of Archæopteryx and the Odontornithes has been amply fulfilled. And as concerning the palæontology of the vertebrata it may be remarked that since Huxley's memorable *Anatomy of Vertebrated Animals*, no vertebrate text-book has accorded the subject fitting recognition. In the work before us, the aforementioned important topics receive due treatment, and ample allusion is to be found to the splendid series of mammalian remains described from the United States deposits by Cope, Marsh, Osborn, Scott and Wortmann, and their associates, as to the work of Marsh, Dollo and Hulke on the Dinosauria; of Seeley and Newton on the Anomodontia; of Moreno and Lydekker on the fossil vertebrata of the Argentines; of Ameghino, Forsyth Major and others, which with them rank foremost in interest among morphological discoveries of recent years having tangible and far-reaching results. Nor has the author of the present volume been behind his fellow-workers in contributing to this end. His investigations on and classification of the fishes are now universally recognised and adopted, and it is in no sense derogatory to the text-books of Palæontology by Nicholson and Lydekker and Zittel, which since the memorable treatise by Owen have been foremost in the field, to say that the present one for the first time places the student in possession of a work in which the subject is laid before them in a concise and readable form, not over-

weighted with stratigraphical considerations. The author's name is itself a guarantee of thoroughness and scholarly research. His Museum catalogue, and the miscellaneous articles which in the intervals of more serious work have fallen from his pen, led us to expect a really good book, and in this we have not been disappointed.

Four hundred and ten pages of the work are devoted to a systematic treatment of the vertebrata taken in ascending order of classification; and of the thirty pages which remain, eight are given over to an "Introduction" dealing in a comprehensive manner with certain broad principles, and the concluding twenty-one to a consideration of "the succession of the vertebrate faunas," the whole closing with a most admirable bibliography arranged on a novel scheme which facilitates reference. In the concluding section of the book the author deals with questions of geographical distribution as bearing on the isolation of continental areas, and upon this, especially as concerning the American continent and supposed isolation since cretaceous times, we cannot altogether follow him, since his theory is in part irreconcilable with facts of distribution of living mammals.

Among the classificatory systems adopted by the author, we hail with satisfaction those of Cope, and that of the Rhychocephalia by Boulenger; and it was to have been expected that the fishes would be arranged in accordance with his own recent scheme, fully developed in his British Museum catalogues. We cannot allow to pass unnoticed, however, his bold persistence in the association of the pteraspid, cephalaspid and pterychthyid forms (with a caution it is true) with the cyclostomi, rendered the more dangerous as it is by the unwarrantable statement, in defining the class, that "it is generally admitted that the existing cyclostomes or marsipobranchs are . . . degenerate". Nor can we see sufficient justification for the continued association of the Coccoosteids (Arthrodira) with the Dipnoi. As concerning the author's diagnosis of the latter, it is a remarkable fact that neither he nor any other distinguished palæontologist who has thus far written upon them appear to us to have sufficiently emphasised the really distinctive character of the group, *viz.*, that the fact that "the dentition has always been confined to the inner bones of the mouth" is the expression of preponderating development of the palatopterygoid over the premaxillo-maxillary arcade, the precise converse of that occurring in all other gnathostomata. And, concerning this, we would point out that from a knowledge of the Stegocephalia our living Urodela would appear to have undergone an essentially similar change (*Tylototriton* alone excepted), and to be therefore in respect to their jaw apparatus as to their digital skeleton one and all degenerate (a conclusion based on the facts of palæontology which the study of the living alone would not suggest), and that consequently we may reasonably suspect the existence of premaxillo-maxillary bones in a vestigial condition among the earlier Dipnoi, especially if the arguments of Dollo in defence of a belief in their crossopterygian origin be, as we consider them, sound.

Since the author gives prominence to morphological considerations, we the more regret his too ready adoption of Hasse's classification by vertebral characters, which has always appeared to us somewhat premature and overdone, and also of the full details of Bashford Dean's description of the Cladoselache fins, which we consider insufficiently established; while, in connection with the question of median fin supports, we could have desired reference to the important observations of Bridge on those of the recent Teleostomi. Mention might also have been made of the temporal fossæ of *Loxomma*, as an important structural departure among the Stegocephalia, and of the compound nature of the sacrum in the Palæobatrachidæ, as also of the remarkable Italian fossil Anura, described by Portis in 1885. And, again, in diagnosing certain of the Reptilian orders, greater stress might have been laid on the forward extension of the pterygoids to meet the Vomers (as, for example, in the account given of the Plesiosauria on p. 159, as compared with that of the Ichthyosauria on p. 177), which all recent investigation has shown to be a character lying at the root of the Sauropsidan series.

In questions of nomenclature the author is not always consistent, for in the use of synonyms and choice of names he has not adhered to any fixed rule. For example, if "Predentata" being admitted as a synonym for the Dinosauria Ornithopoda, why not Pterosauria for the Ornithosauria and Amphibia for Batrachia (justly revived on grounds of priority) since these have long come into general use? And as regards some of the author's determinations of affinity, we fail to see the reason of his action; as, for example, in the placing of the *Prothylacinus* of the Patagonian Tertiary among the Creodonta. The Tillodontia are retained among the Rodentia, and the remarkable suggestion is implied that the Creodonta may have originated among lowly mammalian forms in the Trias or Jura. These are among the more questionable topics which call for comment and reconsideration, together with an occasional misuse of technical terms, as, for example, the reference to an "abdomen" in a fish (*i.e.*, a distinction between abdominal and caudal regions of the body)—matters all of them which mostly affect the interpretation of parts and mere differences in opinion as to ideas and the limitations of terminology. They do not in the least detract from the value of the work, which must rank foremost among current text-books of vertebrate palæontology. The discovery during the last half decade of *Nesopithecus* by Forsyth Major, of *Pithecanthropus* by Dubois, the description by Traquair of *Palæospondylus*, like the still more recent announcement by him of Silurian shark remains and by Marsh of an apparent amphibian footprint in the Devonian, show that vertebrate palæontology is full of promise for the future, and we rejoice in the present book as one that will do much towards reinstating this most tangible, but none too well-recognised, branch of Morphology.

G. B. E.

The Principles of Biology. By Herbert Spencer. Vol. i. Revised and Enlarged Edition. London: Williams & Norgate, 1898.

It is with great pleasure that we welcome the appearance of the first volume of the revised edition of Mr. Herbert Spencer's *Principles of Biology*. It is hardly possible to overestimate the effect exerted, some thirty-four years ago, by the first edition of this important work, and if it is now less familiar to the younger generation of students, this is chiefly due to the rapid growth of knowledge in all departments of Botany and Zoology which tends to render earlier general works less able to reflect the trend of current thought.

The new edition, however, is greatly enlarged and brought up to date, and will form a delightful and indispensable occupant of the library, not only of the biologist, but also of every one who is interested in the philosophical problems which are inseparably associated with a study of the living organism. The author, as ever, generously acknowledges the aid which he has received from various persons—an aid which the enormous literature renders it imperative for any one to seek who attempts to synthesise a system of Philosophy on so wide a basis as that covered by Mr. Spencer.





